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APRIL, 1937.

MIND

A QUARTERLY REVIEW

OF
PSYCHOLOGY AND PHILOSOPHY.

EDITED BY

PROF. G. E. MOORE,

WITH THE CO-OPERATION OF PROFS. F. C. BARTLETT AND C. D. BROAD.

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MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—SOME INCOHERENCIES IN SPINOZISM (I.).

BY A. E. TAYLOR.

I HOPE that the title I have given to these pages will not be misunderstood; to obviate any such possibility, let me explain that its words mean just what they say. I am not offering a *redargutio* of Spinoza; like most great philosophers, he has a personality not to be confined within the bounds of any set of formulæ, and a personality is a thing which defies "redargution." What I am concerned with is *Spinozism*, a certain body of principles which Spinoza believed himself to have demonstrated, though, as I shall argue, he has in fact done no such thing, partly because the principles are mutually incompatible, and some of them false, partly because his own *moral* nature was too noble to allow him to be faithful to some of them. If I am at all on the right lines in what I am going to urge, it will be seen that a certain common conception of Spinoza's philosophy is the exact reverse of the truth. His doctrine has often been described, for example, by J. A. Froude, in his study of the *Ethics*,¹ as one to which we may feel a justified but unreasoned moral aversion, but one of which the logical structure is flawless when once its few initial postulates are conceded. I wish to argue that this notion of Spinoza as the terrible impeccable reasoner is completely false, that the iron panoply of logic in which he is imagined to be

¹ *Short Studies on Great Subjects*, I. 339-400.

encased is riven from head to foot by gaping cracks, and that it is just his illogicality which leaves it possible to feel a deserved reverence for so much of his moral and religious teaching. In this matter I feel myself much more in accord with the judgments of Prof. A. Guzzo¹ than with those of the greater number of expositors, and since Prof. Guzzo's work seems to have been little read by students in our own country, I shall not hesitate to use it freely.²

I must explain also that I am not undertaking to refute a consistent and consequent Monism, or 'Panthéism,' if there has ever been such a thing, but Spinozism, a doctrine which may profess to be, and mean to be, Monism, but never succeeds in being what it would fain be. Spinoza, whatever he may be, is no consistent Monist or 'Panthéist', for the simple reason that he never succeeds in making his *substance*, or *God*, the one and only reality; his whole scheme of physics, psychophysics, and ethics stands or falls with the recognition that there really are such things as the plurality of bodies and minds believed in by the common man. It is true that he *says* that all of them are 'modes' of his one substance, but it is no less vital to his doctrine that these modes shall be really distinct modes with an individuality of their own which is never explained. Peter's mind and Paul's mind may be both modes in the 'infinite intellect of God', and Peter's body and Paul's body both "modes of God as extended", but it never occurs to Spinoza to deny that the "ideas" which make up Peter's mind 'belong together' in a way in which an 'idea' in Peter's mind and one in Paul's do not, or that the components of the 'complex mode of extension' which is Peter's organism form a whole in a way in which components of Peter's body and components of Paul's do not. Peter's head and Paul's trunk do not constitute a single 'complex mode of extension', as Peter's head and Peter's trunk do, nor does an 'idea A' which is part of Peter's mind contract an association with one which is part of Paul's mind. A really consequent Monism would have to regard the very distinction between one body, or one mind, and another as an illusion of

¹ *Il Pensiero di Spinoza*, 1924, a work to which I never recur without illumination and refreshment.

² Suspicion of Spinoza's logic ought to be at once suggested to any reader of the *Ethics* by his fondness for 'proofs' which are said to be immediately evident from a definition, and for the use of *reductio ad absurdum*. The former are no *proofs* at all, but a pretence that proof is unnecessary, and the latter method is almost always fallacious, since there is nearly always an 'omitted alternative' of which the so-called proof takes no account.

"imagination"; a consequent 'Pantheism' would have to deny that there *is* anything at all besides its *ἐν καὶ πᾶν*.¹

But this doctrine, true or false, is not Spinoza's. *Tat tvam asi* is no catchword of *his* ethics, any more than of those of a Southern European Christian priest or a Cartesian vivisector of the "dumb animals". Schopenhauer *is*, if you like, a consistent Monist, since he holds that my *natura*, your *natura*, and the *natura* of all the creatures round us really is one thing; it is the same *natura*. Spinoza is so far from believing anything of the kind that it is fundamental in his *Ethics* that my *natura* is not even the same *natura* as that of my fellow-man; as he explained to Blyenbergh, if Nero commits matricide and Spinoza does not, the reason is simply that matricide "agrees with the *natura* of Nero, but disagrees with the utterly different *natura* of Spinoza".² The very *crux* of his position is that he proclaims in one breath that there is a single all-embracing individual substance, and in the next that every one of us, though we have all just been declared to be mere 'modes' of this single individual reality, is so much of an individual as to be literally *sui generis*. The position seems logically quite untenable, but tenable or not, it is plainly not genuine unadulterated Monism. A really thoroughgoing Monism would have to dismiss the *facies totius universi*, which Spinoza told Tschirnhaus is an 'infinite mode mediately produced by God', as a mere illusion. In fact, it is only a very short step from this recognition of the indefinitely variegated *facies totius universi* as a reality to Pluralism. You have only to insist strongly enough

¹ It might be made the test of a real 'Pantheism' whether it can, like Schopenhauer, accept the Indian *TAT TVAM ASI*. Spinoza notoriously cannot. His whole scale of ethical values turns on the thought that the *natura* of A is always radically *different* from that of B, and therefore the "pleasures" of A and those of B (e.g., the sexual enjoyment of a man and of a horse) are different in kind (*Ethics*, III. 57, Schol.). Indeed, if the nominalism he professes, for example in his correspondence with Blyenbergh, is to be taken strictly, since the *natura* of any two men are radically discrepant, the pleasures which two *men* derive from gratification of the 'same appetite' should also be different in *kind*, though this has, of course, to be conveniently forgotten when he is constructing a general psychology and an ethics. The denial that a "common nature of man" is more than an empty name really removes Spinozism *further* than orthodox Christianity from the thought of *ἐν καὶ πᾶν*. (The same absolute reality of a plurality of different *naturæ* is equally implied as the foundation of Spinoza's political theory of *jus*.)

² *Ep. XX*. Quantum ad me, ea omitto, vel omittere studeo, quia expresse cum mea *singulari natura* pugnant. Thus the *natura singularis* of Nero or Orestes (who figure as illustrations in the discussion) is something really diverse from that of Spinoza, the distinction is not merely a *modal* one, in the scholastic and Cartesian sense.

on the point that, however true it is that the *facies totius universi* remains in some sense the same, it is equally true that it is perpetually changing, and "substance" will become, almost before you are aware of it, a mere collective designation for the complex of *naturæ* which constitute a "multiverse". And I cannot help believing that if Spinoza's life had not been so untimely cut short, he would have seen himself that his actual position was untenable, and that a choice would have to be made between adhering strictly to the unity of substance at the cost of making substance a wholly unknowable "thing-in-itself", and abiding by the knowability of substance at the cost of admitting a plurality of substances.

The real source of the trouble—as of most of the worst metaphysical difficulties in the system—is the fatal admission that there are such things as those which Spinoza calls the 'finite modes' of his infinite substance. We could, I think, just make shift to understand how the one substance expresses its *essentia* in a plurality of 'infinite modes'. We could, for example, understand the statement that extension has a uniform character which is disclosed to us by our study of the unlimited variety of the various geometrical figures possible in it. An Euclidean space has a distinctive character in virtue of the postulates which define it, but the only way to get an adequate insight into this distinctive character is to study the properties which belong, in virtue of these postulates, to triangles, parallelograms, circles, hyperbolas. . . . But all the 'modes' which come under our study in this way, as we extend our acquaintance with geometry, are the successive *determinants* of the *determinable* 'extension'. They are all 'universals', 'high abstractions'; none of them is genuinely a *this* and an individual, or, to put the same thing in another way, none of them has a history, and this is just why it is so easy to know them *sub quadam specie æternitatis*. The utmost degree of specification which can be reached on these lines is at best the *species specialissima*. It is quite different when we have to deal with the so-called 'finite' modes that are the 'particular things' of every-day parlance, which are all really individual, and have a real history. Peter, Paul, this horse, this crystal, are not simply further and more highly determined determinants of the determinable *man*, or *quadruped*, or *mineral*, in the way in which the two fundamental triangles of Plato's *Timæus* are merely further specifications of 'the right-angled triangle'. Each of them is, what the 'isosceles right-angled triangle', for example, is not, a *τὸδε τι*. Now, as I say, we can perhaps understand that Spinoza's one substance should, as a consequence

of its 'essential nature', and of nothing else, find expression in a vast plurality of specifications, so long as these specifications remain at the level of 'high abstractions'.¹ But when we come to the 'finite' modes Peter, Paul and the rest, we are dealing with genuine historical individuals with 'biographies', and we cannot evade the question what provides the principle of their individuation. There is nothing to account for it in the professed postulates of *Ethics* I., and Spinoza never touches on the subject. He is content simply to assume the reality of such genuine individuals as a fact of common experience, and to leave us to guess, if we can, how it is to be reconciled with his avowed principles.

For my own part, I confess that I do not see how the reconciliation is to be achieved. Substance, we must remember, is supposed to give itself its own specifications wholly from within, and the only clue we are ever given to the nature of the process is that it is illustrated by the connection of the specific properties of particular geometrical figures, such as the triangle, with the *essentia* pervasively characteristic of extension. (Thus I suppose we might see an example of a mode and its connection with the attribute of substance to which it belongs, in the proposition that it follows from Euclid's postulates that the three perpendiculars from the angular points of a triangle to the opposite sides are concurrent, or that, if two chords of a circle intersect, the rectangles contained by their segments are equal.) But truths of this kind involve no reference to true individuality at all. They might help us to understand the articulation of a geometrical system; what they leave wholly unintelligible is how *history* gets into the system. It is got in, in fact, simply by taking over the individual things and persons of our every-day thinking bodily, renaming them 'finite modes' of substance, and refusing to ask whether it is in keeping with the avowed principles of the system that there

¹ And yet *can* we understand this after all? Take, e.g., the conception of *res extensa* as a something of three (or if you like, of *n*) dimensions. Is there anything in the *concept itself* to suggest the notion of the various figures of geometry? Spinoza might have profited by the advice given in the next century to Priestly by a bishop, to read the *Parmenides*. If you start your metaphysics with a single 'high abstraction', you will never extract anything from it except itself. No wonder that Spinoza has to admit (*Ep.* 9) to De Vries that he has really given one and the same definition of both *substance* and *attribute*. (Descartes, I think, escapes *this* difficulty. Substance is, with him as with Spinoza, *id quod in se est*, but not at the same time *id quod per se concipitur*; it is 'conceived' through an *attribute*. Thus God, for Descartes, is the substance *par excellence*, and is *per se*, but is conceived by us through the attribute of *cogitatio*, as *ens cogitans*.)

should be such finite modes at all. It is true that in the essay on the *Amendment of the Intellect* Spinoza had expressed his belief that the method of which he was dreaming would get rid altogether of abstractions and 'universals', replacing them by 'singulars' which were somehow to do the required work of disclosing necessary connection without paying the price for it in abstraction. But even if such a method were possible at all, it seems at least clear that Spinoza never attained to it. All through the *Ethics* he is reasoning in terms of abstractions while he supposes himself to be dealing with concrete individuals. He never even realises that a general property, e.g. of the circle, is not made into an individual fact by simply illustrating his reference to it by a woodcut, like that used in *Ethics* II. 8.¹ One might, indeed, fairly suggest that the very possibility of "finite" modes is already excluded by Spinoza's own line of reasoning in the *Short Treatise*. It is argued there (Ch. 2, p. 21, tr. Wolf) that the infinite substance cannot have given rise to any finite substance, because if it had done so, "having been infinite it would have had to change its whole essence". Now, this reasoning, if valid, does not depend on the use of the word *substance*; it applies equally to an infinite "mode"; if an infinite mode is to originate finite modes within itself, it also, in doing so, must change its very character as infinite. (It would be a mere juggling with words to offer the retort that the difficulty does not occur in the case of modes, because a mode has no *essentia*—*essentia* is always the *essentia* of a substance; since Spinoza himself constantly speaks of that which does, or does not, constitute the *essentia* of things which he professes to regard as finite modes, such as the body, or the mind, of Peter or Paul, or what he incorrectly takes to be a "particular" triangle. If there is a single infinite individual, and this individual can give rise to no others, plainly there are no finite individuals at all, and it is only by an illegitimate resort to "imagination" that I make the separation between two complex modes involved in the statement that one of them is the organism of Peter and the other the distinct organism of Paul. To be consistent with my Monism, I must say that the dividing line I commonly draw between the bodies, or the minds, of Peter and of Paul is only drawn by an arbitrary fiction. But though Indian thinkers have apparently been willing to say this, Spinoza was not, and could not have said it

¹ We may or may not be satisfied with any of the theories of the Schoolmen about the 'principle of Individuation', but they were at least aware, as Spinoza seems not to be, that there is a problem to be solved, and they seriously tried to solve it.

without making nonsense of his whole ethical superstructure. Peter and Paul *have* to be for him just as much genuine individuals as they are for the 'man in the street', and the fact is only verbally disguised by re-naming them both 'modes'.)

However, let us waive this point and concede, for the sake of argument, that infinite substance can have *real* finite modes, each of which is really one, and really distinct from every other. Even so, we are only at the beginning of the troubles created for us by the existence of these modes. How grave these troubles are is seen by a consideration of the two fundamental propositions that (1) "substance" is their cause, but an immanent, not a transitive, cause (*Ethics* I. 18); and (2) that the attributes of this substance are, each of them, really indivisible, and taken to be divisible only by an illusion of the imagination (*Ethics* I. 15, Schol. The case is argued with special reference to the "attribute" of extension, but the reasoning is equally applicable to any other.)

The difficulty about causality has been so admirably put by Guzzo that I must begin my remarks by reproducing the substance of his criticism. In ordinary life we constantly constate the connection between a change α in one thing *A* and a change β in a second thing *B*, and say that *A* has caused β in *B*. The causality here asserted is transitive, because the cause is one thing *A*, and the effect a state β in a different thing *B*. Spinoza would have us mend our speech by saying not that *A* causes β in *B*, but that *God*, as determined by the finite mode *A*, causes β in *God*, as determined by the second finite mode *B*. Now verbally, by this formula, we keep within the bounds of *immanent* causality, since God is verbally referred to as both cause and effect. But the device is, after all, only a verbal one for 'saving face'. The patent fact, of which we have to devise some account, is the connection between α , an occurrence falling within *A*, and β , an occurrence falling outside *A*, in *B*. What account of the fact are we to offer? Shall we say that it is the infinite substance itself, as such, which is directly responsible for this connection? If we do, we have broken completely with one of Spinoza's main tenets, that the cause of a finite mode is always 'God as determined by another antecedent finite mode' (I. 28). Or to avoid this difficulty, shall we say that the transaction is due altogether to the finite mode *A*? Then we are ascribing to the finite modes a connection which falls outside the activity of the infinite substance, and are denying another fundamental thesis of Spinozism, that whatever is at all is "in God" and can only be conceived "through God" (I. 15), since we are recognising in our finite modes a transitive activity which falls outside the purely

immanent activity which is all Spinoza will allow us to attribute to "God". (It is really the same difficulty which is exposed by Martineau in a rather different terminology when he argues that the 'finite' modes "institute a *new kind of causality*, other than that by which property depends on essence, *viz.*, that by which *thing comes from thing* . . . they institute a *new order of nature*, other than the order of rational necessity calculating from the dual attributes, *viz.*, an order of scientific experience, spreading a network of *successional connection* through all dimensions of the world".¹) And there is no conceivable escape from this *impasse* when once finite individuals, however cunningly 'camouflaged' as "modes," have been allowed to get a footing in the system; with them transitive causality inevitably makes its appearance, and the immanentism which was to be the central conception of the Spinozistic philosophy is ruined. There are, in fact, only two possible options; either finite individuality is real and transitive causality is real along with it; or else transitive causality is an illusion and there are no finite individuals, only at most "infinite modes", and the determinable of which they are determinants becomes itself no more really individual than "infinite Euclidean space" is individual. Spinozism is no more than a gallant but hopeless attempt to "have it both ways" which ends by having it neither way.

How hopeless the attempt is, is most readily seen from the glaring contradiction on which Martineau, in particular, has fastened. On the one hand, every detail of existence is to follow from the *essentia* of 'God' with exactly the same 'necessity' with which the properties of the triangle follow from its *essentia* as assumed to be formulated in its definition, and for that reason there is really neither contingency nor freedom (except in a Pickwickian sense) anywhere in the universe (I. 29); on the other, the *essentiae* of the "things created by God" do not "involve existence" (I. 24), which must mean that the existence of these things is contingent.² Strictly speaking, to ensure consistency, there ought to be no "things created by God"; there should be only the one universal substance, an infinite determinable inseparable from its no less infinite determinants, and the only *essentia* there is should be the *essentia* of this substance as constituted by its infinitely numerous disparate attributes, and this *essentia* should carry with it necessary existence; a "finite

¹ *Types of Ethical Theory*,² I. 318.

² And yet, when we get to Pt. V., we shall be told there (Prop. 6) that mastery over our passions must involve our thinking of *all things* as necessary, *i.e.*, as being just what according to I. 24 they are not.

mode" should be a mere arbitrary figment of "imagination" without genuine existence and without *essentia*. But when once these modes have intruded into the system, since it is so manifest that their existence does *not* follow from the nature of substance "as the properties of the triangle follow from its definition", there is nothing for it but to make that existence in fact contingent while you are verbally proclaiming that contingency is an illusion of human ignorance.

Indeed, Spinoza's case is even worse than I have so far made it appear. It would not be true even of his "infinite" modes that they follow from the nature of substance as the properties of a geometrical figure follow from its definition. As Tschirnhaus told his master "from the definition of a figure you can never deduce more than *one* property".¹ He would have been still nearer the mark if he had said "you can deduce none". No mere manipulation of a definition of a figure will ever elicit from it any property except that which you have already assumed for the purposes of your definition. What you require as the premisses from which you are to deduce the properties of a triangle are the whole body of postulates which together define the universe of discourse to which the triangle belongs, a region of space (Euclidean or otherwise, as the case may be). Even so it is with the deduction of anything from the nature of "substance". It has, we are told, infinitely numerous attributes, and each of them, in its own way, expresses the same *essentia*. Be it so, and define each of such attributes as is known to us as carefully and exactly as you please; from your definition of *extensio* or *cogitatio*, taken as a sole ultimate premiss, you will extract nothing but itself. That Spinoza should habitually write as though this had never crossed his mind until Tschirnhaus stated the difficulty, seems to me to show that he had not the same insight into 'geometrical method' as the philosophers who have been real mathematicians, Plato, Descartes, Leibniz.²

¹ Ep. 82.

² Descartes, it will be remembered, had specified as the subject-matter of *Mathesis universalis, ordo et mensura* (*Regule*, IV.). Now, both *ordo* and *mensura* presuppose a *real* plurality in that which is ordered or measured. Spinoza has involved himself in the awkward contradiction that though he professes to have demonstrated that there can be only *one* substance, 'God', it is, as he more than once says, *improprie* that God is called *unus* or *unicus*. And he can hardly escape by falling back on the scholastic distinction between the 'transcendental' and the 'numeral' senses of the word *one*, since the thesis which is indispensable for the whole of the subsequent construction is that of I. 5, that 'there cannot be *two or more* substances of the same nature or attribute', where it is precisely *numerical* unity that he is anxious to assert.

It might look at first sight to be a proper reply to the last paragraph to say that I have there myself spoken of the body of the postulates of a geometry as *defining* a space, and thus seem to be conceding that, after all, the infinitely numerous propositions of such a geometry do all follow from a definition, *viz.*, from the definition of a space, or a region of space. But it has to be noted that one cannot define a space directly; one can only reach a quasi-definition of it by making a set of postulates not about *it*, but about specific configurations in it. The assumption, for example, which discriminates the space of Euclid from that of Lobachevsky or Bolyai has to be stated as a proposition about parallel straight lines, or about the angles of a quadrilateral. You have to say that Euclid assumes that only one straight line can be drawn through a given point in a plane parallel to a given straight line, whereas the other two geometers assume that two such parallels can always be drawn; or that Euclid assumes that straight lines in a plane which are not parallel always intersect, the others that there are straight lines which are neither intersecting nor parallel; or again that Euclid assumes that if three of the angles of a quadrilateral are right, the fourth will also be right, the other two geometers that it will be acute. However you prefer to express the critical postulate, *space* is never named in it, and your proposition could not be admitted as a definition of space, or part of such a definition, by any philosopher who, like Spinoza, regards it as self-evident that every proposition must assert a predicate of a subject. (If that position is called in question, there is no longer any reason why, if there is a supreme infinite substance and also a multiplicity of finite things, the finite things *must* be unsubstantial adjectives of the primary substance. They may equally well be, as Christian orthodoxy teaches that they are, "created substances".)

The denial of transitive causality, then, seems to me a position which could only be consistently adhered to in a philosophy prepared, as Spinozism is not, to regard the existence of finite individuals as a mere illusion. Equally unfortunate, to my mind, is the persistent attempt to deny that extension, in particular, is really divisible, or has parts. Of course we see why Spinoza finds it necessary to insist on the paradox. We cannot deduce extension as a consequence of anything more ultimate. (It has generally been held that Leibniz's strenuous attempt to deduce it from differences in 'point of view' among his unextended monads really presupposes in its premisses the very difference of position in space it is intended to explain.) But since there is at least one other 'irreducible' with which a philosophic account of

the universe has to reckon, *cogitatio*, we cannot identify extension with the all-inclusive substance; it must be an attribute of that substance. As an attribute it must express the *essentia* of substance, and, as substance, it is held, can be shown to be one, the attribute must express this unity; it must be rigidly one too. But though Spinoza asserts this consequence hardily enough in words, the facts are really too much for him. No one who is about to plunge into a psychophysical construction like that of *Ethics* II. can get away from the recognition that my body and yours both have their environments, that your environment is not mine, any more than your organism is mine, that both our organisms are exceedingly complex. All through the psychophysical and ethical part of the treatise we consequently find the expressions *bodies*, in the plural, and *parts of body* recurring at every turn, as though the author had forgotten his own declaration that the *res extensa* is only supposed to have parts, or regions, by an illusion of the imagination. It is not surprising, then, to find, as Guzzo has noted, that Spinoza has no sooner made this declaration than he feels it to be an extravagance, and tries to water its meaning down until it becomes a mere common-place. He first reduces the proposition to a mere assertion that the extended is a continuum (which is all that his illustrative woodcut implies). But a continuum, of course, is so far from having no parts or being indivisible, that it is only a continuum because it has an infinity of distinguishable parts; it would not be a continuum if it could not be divided and subdivided endlessly.¹ Finally, we get a still further frittering away of the meaning of the paradox. As an example of the indivisibility of extension we are told that water, considered simply as water, is the same everywhere throughout its whole extent. This is only to say that all water has the same chemical or physical components, that any portion of it behaves like, exhibits the same characteristics as, any other part, a statement which obviously does nothing to justify the assertion that water is only supposed by mistake to have parts, or even the less drastic assertion that a body of water must be a continuum.²

The metaphysical framework of *Ethics* I. thus requires, if it is

¹ Unending divisibility is notoriously not a sufficient condition for continuity, though a necessary one. But it was the only condition recognised in Spinoza's day, and the appeal to it is enough for my immediate purpose.

² *E.g.*, according to Descartes no body of water is really continuous; it always has 'pores' filled with something other than itself. But it would not follow from this that one sample of it will not behave like any other, since the matter in the 'pores' of both might be alike.

to be retained, the admission that "finite modes" are mere illusions, while the whole edifice of doctrine elaborated in *Ethics* II.-V., demands that they shall be nothing of the sort.

This is a bad omen for the consistency of what is yet to follow, and, in fact, there are equally grave inconsistencies yet to be faced. We are confronted with one of the worst of them in the very opening propositions of *Ethics* II. about the nature of the mind and its relation to the body. The body, we are told, is a "complex mode of extension (a finite one) in God", and the mind is a "complex idea, or mode of thought in God" corresponding exactly to the body. We remark at once, then, that Spinoza simply identifies a man's mind with the series of his acts of cognition and volition; it is they which make up the complex mode of which he speaks. Commonly we distinguish three different things: (1) an act of thinking, (2) the man or person who thinks this thought, or, to speak more accurately, though less idiomatically, thinks this *thinking*, (3) that which the thinking thinks of; or more briefly, we distinguish (1) a *concupient*, if I may invent such a word, (2) a *conception*, (3) a *conceptum*. Spinoza simply identifies (1) and (2) without more ado. We commonly hold that there is such an entity as the man who does the thinking, and if we are to be proved wrong about this, we are at least not proved to be wrong by simply taking the unreality of the distinction for granted under cover of an arbitrary definition.¹ When I say that I am engaged in a certain train of thought, I mean, or suppose myself to mean, something more than that such a train of thought is an actual occurrence, or even that it is connected in an obscure way with the concomitant occurrence of a certain train of cerebral processes; both those statements might be true, and yet—or so, at least, all of us commonly think—I might be non-existent. What I suppose myself to mean by *I* is neither the mental events nor the bodily events but the man, or self, who owns both the mind and the body. If this is to be shown to be a mistake, it must be shown to be so by a more refined and careful analysis of *self-consciousness*. But Spinoza has no improved theory of self-consciousness by which to correct the *naïveté* of our common thinking. He merely ignores it. No

¹ And we are certainly not proved to be wrong by a magisterial reference to Kant's assault on the 'paralogisms' of Rational Psychology. Kant's attempt to identify the thinker with the 'I think' which is a mere formal concomitant of thinking (or at least to argue that the identification is possible) seems to me a particularly glaring example of the 'fallacy of misplaced concreteness'. He offers us a 'high abstraction' as the complete analysis of a concrete historical fact.

doubt he is careful to tell us that just as there is an "idea" in God of every mode of extension, so there is a second idea of every idea, and this is apparently meant to be his account of consciousness of self. But as Martineau rightly said, what is 'explained' by this theory of the "idea of the idea" is not what everyone means by self-consciousness: it is not the *man* who is said to be aware of *himself*, but his *ideas* which are asserted to be conscious of *themselves*. And the two things are very different. It is one thing to say that I know the Pythagorean theorem and also know that I know it; it is a wholly different statement to say that my *knowing* of the Pythagorean theorem is a knowing that I am knowing it. The first statement is just what all but the "sophisticated" believe to be true, and is thus, at least, highly plausible; the second, so far as it is intelligible, seems to be plainly false. Spinoza's doctrine does not even explain how we could ever have fallen into the illusion of supposing that *we* are self-conscious.

And if it seems at least difficult to subscribe to the identification of concipient and conceiving, there may perhaps be an equal difficulty in accepting the theory of the relation of conceiving and *conceptum* which Spinoza offers us as an account of what we mean by knowing a natural fact. What happens, according to the theory, when, in common parlance, I am said to know a fact about the bodily world? Simply a compresence "in God" of a physical event and a mental event which is its counterpart. But is this enough to constitute knowledge? Manifestly not; on the theory itself there is such a compresence, in God 'as constituting my mind', of an 'idea' corresponding to *every* physical process, in God 'as constituting my body', but of most of these physical processes we are profoundly unconscious. If I am to know the simplest fact about my own body, it is not enough that an 'ideal counterpart' of that fact shall *exist*; the bodily fact and the ideal counterpart—or rather I who am the owner of the ideal counterpart—must further stand in the unique and indefinable relation *known-knower*. Otherwise you might have the closest correspondence between the "modes" of extension and of thought, there might be a determinate *a* in the one for every *a* in the other, and yet there would be no *knowledge* of the bodily world. The changes in that world would unfold themselves in their regular causal order; concomitantly there would be a second world of 'ideas' also unfolding themselves in *their* regular order, but there would be no *cognisance* of terms of the one order by terms of the other. Minds answering precisely to the Spinozistic definition might perfectly well be what Münsterberg held the

"mind" described by the psychologist to be, beings which "know nothing by their cognitions and will nothing by their volitions". Münsterberg, of course, meant his language to be a deliberately paradoxical way of saying that the "mind" discoursed of by the laboratory psychologist is "not the real thing", but a fiction consciously substituted for the reality. But the question I should like to ask is whether any philosopher who deliberately substitutes 'the existential *compresence* of idea and *ideatum*' for "knowledge of the *ideatum* through the idea" has not unconsciously confused the fiction with the reality.

Now this confusion, which turns upon forgetting that an act of knowing is not merely something that 'takes place along with' certain events in the extra-mental world, but is an *apprehension* of those facts by a knowing subject, is, in Spinoza, not a mere casual oversight; it is made almost inevitable by the fundamental *hypothesis* of his whole system. If the various "modes" can be really sorted out as belonging each to an "attribute" of substance wholly disparate from all its other "attributes", and if it is true that no mode of one attribute can ever contribute to the causation (which for Spinoza, means the rational explanation) of a mode of any other, then thought and extension should go each its own way, each "corresponding", if you like, in the closest fashion to the other, but wholly unaffected by it. Since no appeal is to be made to any fact of the one order in accounting for any fact of the other, our psychology, theory of knowledge, ethics should never be allowed to make any mention of the existence of bodies; they should deal exclusively with relations between 'modes of thought', regarded simply as 'mental occurrences' without any objective reference. We ought to be able to construct an epistemology which should treat of knowing without ever introducing the reference to anything non-mental which is a known object. And in doing so we should be adhering strictly to the line of thought which led Spinoza to define an adequate idea as one which has the internal characteristics of truth, and to exclude all reference to the currently recognised external characteristic, 'agreement of the idea with its object'. We should have, what it seems to me later philosophers have never succeeded in attaining, a thoroughly coherent 'coherency' theory of truth.

In point of fact, however, it is so abundantly clear that no account can be given of knowing with the least vestige of plausibility which ignores the most patent characteristic of knowing, *viz.*, that it is always the knowing of an object other than itself, that Spinoza himself habitually neglects to observe his own rule that the modes of each attribute are to be explained

exclusively by reference to other modes of the same attribute. To account for the errors into which we are betrayed by 'imagination,' he is forced to fall back on the thesis that our 'ideas' of an external body, until they have been corrected by the teachings of philosophy, represent rather the state of our own body, as affected by the external body, than that of the external body itself (II. 17, Schol.). The states of my own body are thus openly introduced as the really significant determinants of my 'ideas'. For example, I have an hallucination of the presence of Peter's body in some situation in which Peter's body is not really there; this is explained by the usual 'associationist' theory that I imagine Peter to be present because, in consequence of 'association', certain parts of my brain are affected as they would be if Peter were actually there. We need not quarrel with the explanation, but it is flatly incompatible with the previously assumed doctrine that modes of 'thought' must be accounted for exclusively in terms of other modes of 'thought'. It has been now recognised that, after all, there are modes of 'thought'—those involved in sense-perception—which cannot be accounted for except by reference to modes of 'extension'. And this necessary admission is what really gives rise to the formidable difficulty raised by Tschirnhaus,¹ why, if there are an infinity of 'attributes' and every 'mode' is represented in each of them, our knowledge should be confined to modes of thought itself and modes of extension. It might have been a consistent position—though it would have made knowledge of the extramental world impossible—to say that a mode of thought can be cognisant of nothing else but modes of the same attribute. But the moment it is admitted that a mode of thought can be the apprehension of a mode of extension, it becomes pertinent to point out that on Spinoza's principles the given mode of thought a_1 is related to the corresponding mode a_2 of extension *only* as it is also related to $a_3, a_4 \dots a_n$, the corresponding modes of all the "unknown attributes". The relation in each case, is simply that of compresence and correspondence (whatever correspondence may mean). No reason has been given for holding that there is any further and more intimate relation between a_1 and a_2 than between, say, a_1 and the 'unknown' a_3 . In the case of a_2 the mere fact that it is compresent 'in God' with a_1 and, in some unexplained way, corresponds to a_1 , was supposed to be equivalent to the fact, ' a_2 is known through a_1 ': by parity of reasoning, seeing that the same conditions are fulfilled for a_3, a_4 and the

¹Ep. 65.

rest, it ought to follow that $a_3, a_4 \dots$ are also known. Spinoza's reply (*Ep.* 66), as we know, was that they *are* known, though not by *our* minds. But this is a mere evasion of the difficulty. Since my mind admittedly can get at and apprehend what is not mental at all, as it does when it knows any fact about body, what is there to prevent it from getting at the whole range of non-mental modes? *Why* should it be able to burst the barriers which separate attribute from attribute in the one case of extension and in no other?

I own I suspect Spinoza of having fallen in his reply into a fallacy which would naturally be facilitated by his identification of the mind with the complex of 'ideas'. There *are* ideas of all the modes of all the attributes, he says, but these ideas 'constitute the minds of some other beings, not our minds'. Now it is true, of course, that the thought or perception of a mode a_3 of some unknown 'attribute' which 'corresponds' to a_2 , a mode of extension, will not be the *same* thought as the thought of a_2 . But this is no reason for saying that a_2 and a_3 may not be thought of or perceived by the same knowing *subject*. An electrical disturbance in the atmosphere is 'expressed' both by a flash of lightning and a roll of thunder, but we cannot infer that because the visual percept of the flash is a different *percept* from the audible percept of the thunder-clap, one percipient cannot be aware of both. It is not true that every man must be either blind and unable to see the flash, or deaf and unable to hear the crash. Tschirnhaus was entirely right in saying that Spinoza ought to have explained why, out of an infinity of 'modes' which, as expressive of the same fact, stand on the same level only one—the expression in the attribute of extension—is accessible to us.

There are, of course, philosophies to which it need present no difficulty to suppose that there may be creatures whose perceptions are wholly different from ours, revealing to them characters of the external world which we cannot even imagine and concealing from them all which are so familiar to us. But a philosophy which can consistently find a place for such a speculation must be one which is not dominated, like Spinoza's, by the conception of 'attributes', each made up of utterly disparate expressions of the same identical 'modes'. Such philosophies can intelligibly say that if there are percipient creatures such as we are imagining, the range of natural fact disclosed to them and that disclosed to us are simply *different*; the 'modes' they know are *not* counterparts of the 'modes' we know. For Spinoza the problem is insoluble because he assumes that the unknown $a_3, a_4 \dots$ are

really the same fact as the known a_2 , and that further the knowing of a_2 by a_1 is accounted for by the consideration that a_1 is the same fact as a_2 , only expressed under another 'attribute'.

(The same inadequate notion of what is meant by knowing has often reappeared in later writers who might have learned better from reflecting on the insoluble tangle into which it had led Spinoza. Huxley, for example, criticises Descartes' *cogito* by saying that all we are warranted in assuming as indubitable fact is not *cogito*, I am conscious, but *cogitatur*, there is consciousness going on. He should have seen that Descartes proved his greatness as a philosopher precisely by not falling into this trap. His immediate certainty is what it ought to be, a concrete personal fact, *ego nunc et hic cogito, ille homo qui est Renatus Descartes hic et nunc cogitat*,¹ not a mere 'high abstraction'. The only real criticism to which he lies open is that he is content to be sure that *hic et nunc cogito*, where, to exclude all abstraction, he should have said *hic et nunc cogito hoc vel illud*, so preserving the necessary reference to the object of awareness. The concrete fact is never 'awareness exists', but always 'this or that subject is now aware of this or that'.) It is this neglect to insist on the unique character of all knowing as an apprehension of an object by a subject which explains the standing and apparently unconscious Spinozistic equivocation by which 'the idea of Peter' may mean either 'the mental complex which corresponds to Peter's brain and nervous system, the mind of Peter', or 'the mental complex which exists when Paul thinks of Peter', Paul's 'idea' of Peter, or may mean both in the same breath, if it is convenient for the argument that it should. In fact, the two 'correspondences' are of a wholly disparate kind. The 'idea of Peter' which is the mind of Peter 'corresponds' to 'the body of Peter,' in Spinoza's theory, in the sense that it is the same identical thing, expressed first in terms of one 'attribute' and then in terms of another. 'Paul's idea of Peter' does not 'correspond to the body of Peter' in the same sense; in that sense what it 'corresponds to' and 'represents', as Spinoza himself is careful to tell us, is not Peter's body but Paul's body.

¹ And yet this second transcription is not quite adequate. For it only expresses a judgement which *might* be made by a bystander, and *he* might conceivably be mistaken in thinking that 'the man who is René Descartes' is there, or is thinking. And even if the judgement is made by René Descartes himself, he *might* have 'forgotten his own identity'; he might be wrong in thinking of himself as the person known as Descartes, exactly as a lunatic is mistaken when he says he is Julius Caesar or the angel Gabriel. We cannot really dispense with the *ego*, if our statement is to be an indubitable transcript of the immediately certain.

The only sense in which *this* 'idea of Peter' 'corresponds' to Peter's body at all is that it conveys information to Paul about Peter's body, has Peter's body as its *object*, and that when Paul thinks truly about Peter, the information conveyed is correct.

I own that I should find it hard to acquit Spinoza of bad faith if I were not convinced that this prolonged equivocation is quite unconscious; and that it should have imposed on him I can only explain by assuming that this is a consequence of his complete blindness to the real character of awareness, the necessity for a concipient and a *conceptum* as well as for a conception. The trouble in Spinoza's pretended account of knowledge is that the conception and the *conceptum* are confused together by using the same word *idea* indifferently for either, as occasion may serve, and that the concipient, if not disregarded altogether, figures as no more than an empty theatre in which the *ideæ* go through their evolutions. It is precisely the same vicious abstraction which infects so much of the "analysis" which has been so popular among us of late years. We are offered more or less ingenious speculations about the way in which mental operations may be analysed, while it is blindly forgotten that a real operation does not perform itself; it presupposes an operator and a "subject" to be operated on. And this is as true of mental operations as of surgical.

Even so, we have not got to the end of the illogicalities of the second part of the *Ethics*. What the whole of the psychophysics and epistemology are meant to lead up to is an account of the method by which we may rise from 'imagination' with its inadequate ideas, which reveal rather the condition of our own body than the character of our environment, to knowledge. Spinoza himself frankly avows, as Guzzo has noted, that there is a great *lacuna* in the argument just at this critical point. For any detailed account of the transition we are referred (II. 29, Schol. I.) to an unwritten work which appears to be the *Tractatus de Intellectus Emendatione*. Since we only possess the introductory chapters of this projected work, we have to content ourselves with the rather meagre general information given in the *Ethics* itself. All that is said there, in the lemmas appended to II. 13, is that there are some characters which are common to all *res extensæ*, and others which, though not common to all, are common to our own organism and all the bodies of its immediate environment. Since in both these cases there can be no question of the adulteration of our idea of a property by the admixture of elements representative only of the state of *our* body, the 'ideas' of these characters are always adequate, and provide us with a

body of 'common notions' from which we can deduce rigidly true scientific conclusions. It is these common characteristics of all bodies which are apparently meant by that 'adequate idea of God' which Spinoza ends by declaring to be always present in every man (II. 47). When he says that all men have, and have always had, an adequate idea of God, and that in this idea we have the foundation requisite to science, what he seems to mean is that we can deduce the contents of geometry and kinematics from a number of postulates which must be true without reserve, because they express the nature of the attribute *extension*. Now, granted that this were so, it is clear that the science you could deduce from these 'common notions' would never take you beyond the bounds of kinematics; you could not advance as far as a physics and a chemistry, still less would you have any foundation for what Spinoza really wants to construct, an ethic, a doctrine of moral values. And even so, there is a further awkward question rightly put by Guzzo.

According to Spinoza, it is so far from being true, in the words of Plato, that it is 'hard to discover the Father and Maker of all things, and impossible to speak of Him to the multitude', that all men whatsoever always have had the 'adequate idea' of God. How this is to be reconciled with the scornful language used elsewhere habitually by our philosopher about other men's 'idea of God' is his own concern. Presumably he would say, like Descartes, that they all have the true 'idea of God', but most of them confuse it with an irrelevant 'image' of a 'magnified non-natural man', though one would think the confusion ought to be impossible in a mind where the 'adequate idea' was already present. But in any case, the admitted fact remains that most men live at the level of 'imagination'; their 'adequate idea of God', which has always been with them, is inoperative. When a chosen few become philosophers, then, their previously inoperative 'idea of God' must suddenly become an effective force in their mental life. How is this startling "conversion" to be effected? Why, at a certain date, does my 'idea of God', which has hitherto been dormant, begin to dominate my whole thinking? Any answer Spinoza might have attempted would probably have been contained in the missing chapters of the *de Intellectus Emendatione*. But it is abundantly clear that he could not have made the process begin with a freely chosen turning of the mind from darkness to light, since he has expressly insisted on it that subjection to the illusions of 'imagination' is a strictly *necessitated* consequence of our position in the universe; we cannot break our own chains (II. 36, IV. 4). Nor again are we

liberated *ab extra* by the rising on us of a spiritual sun which had previously been hidden by the cloak of night, for *all* men have *always* had the 'adequate idea' of God. It remains, then, an inexplicable mystery why not all of them are at any rate at home in geometry and kinematics. It appears then that, when all has been said, there is no sure way to which we can trust for the 'amendment' of our understanding. Perhaps a man will rise from the level of *imaginatio* to that of *ratio* or *scientia intuitiva*, perhaps he will not, and whether he does or does not is dependent on causes which, if Spinoza's rigid necessitarianism is true, he can neither foresee nor control. It is matter of predetermination, or what comes to the same thing, of pure chance. (For if all that can be said is that what will be will be, it makes not an atom of practical difference whether you call this doctrine absolute Predestination, as Spinoza likes to call it, or Absolute Chance.)

It is not sufficiently to the point to reply that there certainly is a great 'something not ourselves' on which we are dependent, whether we call it the 'divine decrees', or the 'order of nature', and that perhaps no philosophy has ever succeeded in showing how our personal freedom and initiative can be conciliated with this dependence. This may be so, and yet, may we not say? no philosophy can possibly be sound which does not at least recognise the need of the conciliation by admitting the manifest *prima facie* fact of intellectual and moral initiative. By his *a priori* assumption of universal and absolute "natural necessity" Spinoza really runs away from the problem with which the philosopher should grapple, and here he reveals the measure of his inferiority, as a *philosophical thinker*, to Descartes. Descartes' *Fourth Meditation* may fall very far short of being a solution of the difficulty, but at least it honestly tries to take into account all the *prima facie* facts; Spinoza mutilates the "appearances" which it is his business to "save", out of all recognition. It is simply not true to say that what I call acting with freedom means being conscious of my act but unaware of any cause for it (I. 36, Appendix). On the contrary, the very reason why I am so confident that my adherence to a doctrine or my adoption of a line of action has been *free* is precisely that, rightly or wrongly, I am convinced that I do know the cause of the decision, and that the cause is myself.

It is open to Spinoza to argue that this conviction is always mistaken, as it must be if his identification of the mind with its 'ideas' is sound; it is not open to him to confuse this definite conviction of personal initiative with 'ignorance', the *absence* of

information. When Socrates remains seated in the Athenian prison awaiting the draught of hemlock, instead of taking the road to Megara or Thebes, as his friends want him to do, and gives as his reason that *he* judges it "best" to abide by the sentence of a legal tribunal, on the face of it there is no resemblance with the case, imagined by Spinoza (*Ep.* 58), of a stone, conscious of its falling, but unaware of any reason why it should be falling. For a real parallel to Spinoza's consciously falling stone we require to go not to the normal actions of responsible human beings but to the psychology of "deferred hypnotic suggestion". Now I gather from the literature of this subject that though persons who act on such "deferred suggestions" are quite unaware that they are carrying out a movement which has been suggested to them under hypnosis, they reveal, if questioned, that they are puzzled about the reasons for their behaviour, do not "quite know why" they make the movement they do make. But this at once discriminates such behaviour from the action which the ordinary man regards as the proof of his freedom; there he thinks himself free precisely because he believes that he *knows* so well why he does what he does, and is acting on his own personal judgement. *Prima facie*, at least, the two cases are as unlike as they could well be—a philosopher who proposes to explain away the difference between them is, no doubt, entitled to a hearing, but we are also entitled to remind him that the appearances are very much *against* him, and that if he is to prove his case he must do so by fairly dispelling the adverse appearances; he cannot prove it by simply assuming universal determinism.

I would add that he does not make his argument any stronger by merely dubbing the appearances unfavourable to him 'vulgar prejudices' and affecting the tone of a 'superior person'. Spinoza strikes that note far too often, especially in his correspondence, and though we must make all allowances for a man whose feelings had been naturally lacerated by the experiences of his early life, one may suspect that his frequent resort to it is, sometimes, due to an uneasy suspicion of the weakness of his case.

In a word, I would urge that Descartes has the merit of avoiding two grievous errors which are really fatal to Spinozism:

(1) He rightly insists on the *transcendency* of his *summum ens*, making it a source of all other *entia*, but never dreaming of treating it as a subject of which they are predicates; he sees, what I should say is perfectly true, that a philosophy which puts a *summum ens* at the head of things must be a doctrine of Creationism.

(2) Consequently, he, unlike all Spinozists, can be strictly consequent in his theory of nature as a mechanism. If nature is a machine at all, it ought to exhibit the obvious and salient characteristics of all mechanism, that (a) there is always, behind the machine, intelligence, not its own, to construct and operate it; (b) that the machine is constructed with a view to definite work to be got out of it, and that 'mechanism' demands the reality of 'final causality', however presumptuous it may be in us, who only see part of its working, to assume that we know just what the purposes of its inventor and operator must be. Descartes' great machine is not, like Spinoza's, one which constructs and operates itself, and all to grind nothing. But Descartes' rejection of final causes from Physics does not mean that he has any doubt that there is a purpose in creation; it is a mere confession of our inability—apart from revelation—to say what the purpose may be.

(To be concluded.)

II.—AN EPISTEMOLOGICAL APPROACH TO THE SPECIAL THEORY OF RELATIVITY.

BY C. T. KRISHNAMA CHARL.

AT a time when traffickers in knowledge are making the most sweeping claims for a "four-dimensional Universe", it may not be inopportune to consider the embarrassments which Hermann Minkowski's words have created for philosophy. "From henceforth, space by itself, and time by itself, have vanished into the merest shadows and only a kind of blend of the two exists in its own right." This bold and startling utterance has not lost its significance for physical research to-day. Persistent scientific investigation instead of leading us away from the mode of contemplating the world which Minkowski regarded as indispensable has only reaffirmed it as the fundamental basis of our understanding of nature. Physicists and mathematicians whose competence can hardly be a matter for dispute have, during the last quarter of a century, sought to endorse Minkowski's verdict that space and time separately have been degraded into the merest shadows and that only a sort of combination of the two can preserve any reality. It is the thesis of the present paper that the mathematical theory of space-time is by no means unambiguous in the epistemological consequences to which it leads. The identification of the space-time continuum with "objective reality" so commonly met with in popular presentations of the theory of relativity needs to be carefully examined.

(I.)

It is necessary in the first place to refer to Einstein's explanation of the negative result of the Michelson-Morley experiment. If we accept his restricted theory of relativity, we have to surrender the view that lengths and time-intervals are invariant characteristics of nature. Whatever plausibility attaches to this view may be ascribed to the fact that the velocity of light is large compared with the material velocities it is usual to handle in physics. The moment we have recourse to refined methods

of measurement, we see that such a view is exposed to invincible objections and must be definitely abandoned. To mould our thoughts and language in accordance with what has been experimentally established, we have to postulate that every observer carries with him his own space- and time-measures. These measures do not vary in a random way as we pass from one frame of reference to another, but are connected by a set of equations. The constructive part of Einstein's reasoning is determined by the Principle that "the laws by which the states of physical systems undergo changes are not affected whether these changes of state be referred to the one or the other of the two systems of co-ordinates in uniform translatory motion (with respect to each other)."¹

It is important to realise at the outset of our inquiry that the alterations in space- and time-measures brought about by uniform relative motion are true only for some arbitrarily selected "stationary" observer. No experiment conducted on the moving system will disclose the slightest change in the measuring rods and clocks. The fact—and it is not easy to exaggerate its importance—that the "stationary" system can only be verbally distinguished from the moving system is what compels the relativist to regard the "contraction" hypothesis as based on an *ad hoc* assumption, and what leads him to pronounce finally that the changes in the recording instruments are not brought about by "physical causes" but appear for "kinematic reasons". It is the latter view Sir Arthur Eddington is expressing when he declares that the proposition that a moving rod is shortened in the direction of its motion is "true but not really true".

The distinction between "true" and "really true" may at first sight appear too finely spun to be anything except a fruitful source of controversy. We need only remind ourselves here that such a distinction is characteristic of the situation created by relativity. The peculiar "triadic relation" which Einstein's theory imports into the very definitions of lengths and time-intervals has bulked large in epistemological discussions concerning relativity. Physical length, it has been remarked, cannot be defined in a "dyadic" fashion, if physical measurement cannot reveal any unique length which is an intrinsic property of a moving object. Again, if no two observers use the same time-measures, statements concerning "simultaneity" can to all intents and purposes only assume the form: "The event e_a is simultaneous with the event e_b with respect to the

¹ *The Principle of Relativity*, by A. Einstein and others (Methuen), p. 41.

system S." Epistemologists unwilling to relinquish the physicality of length have affirmed the objectivity of what Prof. Lovejoy has termed "respective" attributes. "A character is said to be 'respective' when the term designating it has no meaning, as a possible predicate of a subject of discourse, unless, besides the term and the subject, some definite third term is implicitly or explicitly specified."¹

At the risk of some repetition, we wish to point out that the "respectivity" of lengths and time-intervals postulated in Einstein's theory is not relativity in the trivial sense of the word. It is a commonplace of physics that every measurement is relative to the "scale form" and the unit adopted. This is a matter of technique and in itself is no reason why we should renounce the view that a determinate unique length can be an intrinsic property of a moving object. Physical length can be defined in a "dyadic" fashion (*i.e.*, with respect to all frames of reference) if, when the unit has been fixed, instruments of optimum accuracy yield within prescribed limits of error the same consistent measures of the same object. Physical length becomes "respective" if, when the unit has been fixed, instruments of optimum accuracy do not yield within prescribed limits of error the same consistent measures of the same object. To affirm the objectivity of a "respective" length is to concede the somewhat curious possibility that a measurable property which is a datum only with respect to a particular frame of reference can still be regarded as ascribable to the object of knowledge. It is exceedingly difficult on this view to state satisfactorily what we mean by an extended physical object.² If we define the object exclusively in terms of "respective" attributes, it is easy to see that we shall be delivering the case into the hands of the phenomenalist; for we then suppose that no measurement can reveal any property of the object which is not itself a "respective" attribute. The sceptical insinuation that a physical object is just a system of "appearances" requires to be fairly met, and it is not met by contending that even in the theory of

¹ A. O. Lovejoy, *The Revolt against Dualism*, III.

² It is usual in epistemological discussions concerning the status of sense-data to recognise a kind of "respectivity". The "respectivity" of the "elliptic penny" and the "bent stick" must be so familiar to students of epistemology that it perhaps hardly needs any mention. The relativity theory, as must be evident, is not the outcome of an unbiased scrutiny of "elliptic pennies" and "bent sticks". No difficulties are encountered in physics so long as the definition of the "real" penny or the "real" stick in terms of standard physical operations remains unchallenged.

relativity a physical object can have *some* length in the direction of its motion ; such a statement leaves much to be desired in the way of clearness and, in any case, is surely an evasion of the difficulty. The test of objectivity in physics, it has been said, is invariance for all observers, and the acceptance of this principle leads us to the conclusion that lengths and time-intervals which are not unambiguously ascribable to nature cannot be treated as fundamental characteristics of it.

The ambiguities in the modes of measurement of space and time are mathematically disposed of in the theory of relativity. Minkowski in his famous lecture on "Raum und Zeit" showed that the kinematics of Einstein could be represented in an elegant form by treating time as a fourth co-ordinate along with the three dimensions of space. Minkowski's four-dimensional continuum is metrical like euclidean space, but the quadratic form which determines its metrical structure permits of one negative dimension. Instead of plotting real instants along the time-axis, we may plot the quantity ict where $i = \sqrt{-1}$. By treating time in this way, it can be shown that, while lengths and time-intervals must indeed be "respective" magnitudes for any observer, the "space-time separation" of two events is an "invariant" for all observers. The objective Universe disclosed by relativity, it has been acknowledged on all hands, is a Universe in which space and time are indissolubly linked together, a Universe not of space and time but of space-time. It is rather unfortunate that philosophers who have busied themselves with the paradoxes about lengths and time-intervals should have paid little attention to the problem suggested by Minkowski's imaginary time-dimensions, and it is to be regretted that mathematical physicists preoccupied with technical issues should have more or less systematically ignored epistemological questions. The remark one often hears from the relativist that the introduction of $\sqrt{-1}$ into the mathematical picture simplifies all calculations, is jejune and fails to meet the real difficulty. If the "spatialisation" of time is little more than a mathematical camouflage, it is not easy to understand why we should confer the attribute of "objective reality" on the space-time continuum, and it is not altogether clear how the introduction of four-dimensional events will dispose of the epistemological ambiguities in the concepts of space and time.

The adoption of a formalistic point of view precludes the possibility that $\sqrt{-1}$ can have a significance for the external world and renders it difficult for us to suppose that an imaginary time-dimension is anything but *ben trovato*. And yet, viewing

the problem from an epistemological angle, it is surely no coincidence that Minkowski's world-geometry which resolves the paradoxes about space-measures and time-measures is also the geometry in which the finite constant velocity of light c appears as an inherent property of space-time, as the unattainable but observable upper bound of all material velocities. c enters into the formulation of all physical laws in the theory of relativity and is a fundamental constant of electro-magnetism. It is at least curious that this fundamental constant of electro-magnetism should in the mathematical apparatus of the theory of relativity be linked on to the imaginary $\sqrt{-1}$. Dr. Whitehead in his *Concept of Nature* has pointed out that the formulæ for the transformation of measures made in one system to measures of the same facts of nature made in another system will involve an arbitrary constant k which is of the dimensions of the square of a velocity. He considers four possible cases: k may be zero; k may be infinite; k may be negative ($-c^2$); k may be positive (h^2). The first two cases may be waived. The fourth case yields a possible type of transformation-formulæ but (as Dr. Whitehead is careful to add) "not one which explains any facts of experience".¹ It is pertinent to ask here why the third case alone in which k happens to be negative yields transformation-formulæ adequate to the facts of experience. Prof. Weyl has said that while a negative time-dimension is of no mathematical importance, it may have "a deep significance for reality".²

(II.)

The question—which for an epistemologist is neither superfluous nor unmeaning—as to why $\sqrt{-1}$ should intrude into relativistic descriptions of time Mr. Dunne has recently attempted to answer in his book, *The Serial Universe*. The theory he invites us to consider turns on the assumption that time has a "regressive" character. Every time-travelling field of presentation, if we follow Mr. Dunne, is contained within a field one dimension larger. The time-dimension for a three-dimensional observer is simply the direction in which his field of presentation is travelling in a four-dimensional continuum. An "interval of time" is both a distance travelled and the time taken in travelling that distance. Thus a "second" represents a stretch of space covered by a three-dimensional clock travelling along the fourth dimension S_4 or T_1 in an interval marked on a time-axis T_2 which is perpendicular to T_1 . If we wish to plot on S_4

¹ *The Concept of Nature* (Cambridge), p. 132.

² H. Weyl, *Space-Time-Matter* (Methuen), p. 217.

or T_1 the four-dimensional distance traversed in unit time, we must rotate the axis of time T_2 until its divisions coincide with those of time T_1 . The appropriate unit for S_4 or T_1 will involve an arbitrary constant k . All time-intervals recorded by a three-dimensional clock must be multiplied by ki if we wish to plot the distance travelled by the clock along the fourth dimension S_4 or T_1 in unit time. It is possible on this view to regard k as "the velocity of the 'now'"; for it is the distance travelled by a clock along S_4 or T_1 (measured in appropriate units) divided by the time required for the transit. Mr. Dunne proposes to resolve the paradox about light by equating the finite constant velocity of light in *vacuo* (c) with "the velocity of the 'now'" (k). The treatment suggests that k may be regarded as the upper bound of the velocity of any material body in space.¹

Without in the least pretending that this bald summary of *The Serial Universe* will do justice to all the motives which have inspired its author, we wish to offer one or two criticisms. Mr. Dunne's employment of $\sqrt{-1}$ in rotating the axis of time T_2 presupposes that the "regressive" character of time has been fitted into an euclidean space-scheme. Now the idea that time is merely a fourth dimension of euclidean space contains in itself nothing new. It has been put forward by P. D. Ouspensky, C. H. Hinton and C. Bragdon, to mention only three names. Ouspensky assumes that "extension in time" is extension into unknown space and that, therefore, time is "the fourth dimension of space".² Hinton toys with the same idea in his book, *The Fourth Dimension*. Philosophically viewed, this Hintonian-Ouspenskian theory suffers from an obvious drawback to which Mr. Dunne himself has invited our attention. We cannot consistently assume that movement in time is merely translation along a fourth dimension without introducing the further assumption that an object which moves along the fourth dimension must take time over its travel. The difficulty is inherent in any theory which resolves time into space. The moment we posit movement along world-lines stretched out in a static four-dimensional Universe, the idea of a flow of time reasserts itself. The difficulty is dismissed by Hinton with the facile remark that "the plane of consciousness" can register only cross-sections of the four-dimensional Universe.³ Ouspensky is disposed to welcome the hypothesis of a multi-dimensional continuum as suggesting a way out of the impasse. Mr. Dunne

¹ J. W. Dunne, *The Serial Universe* (Faber), ch. xiv.

² P. D. Ouspensky, *Tertium Organum* (Kegan Paul), p. 47.

³ C. H. Hinton, *The Fourth Dimension* (George Allen & Unwin), p. 25.

meets the difficulty by consistently postulating an "infinite regress" in the time-dimension and by explicitly recognising "higher time-dimensions". The novelty of Serialism lies in this: in a Serial Universe it is permissible to rotate the geometrically mapped-out axis of a time-dimension (T_2) until its divisions coincide with those of a time (T_1) one dimension lower. Multiplying a length by i changes its direction without affecting its intrinsic magnitude. In Serialism the "double character" of a time-interval is the *raison d'être* of the geometrical operation. An interval of time is *both* a distance travelled and the time taken in travelling that distance.

At least one protest seems to be called for here. The Hintonian-cum-Ouspenskian assumption that our "time-sense" is merely an undeveloped or imperfect "space-sense", plausible as it may appear at the first blush, on a closer scrutiny will be found to involve a dangerous simplification of the problem of time. The hypothesis of an infinity of time-dimensions which Mr. Dunne recommends is not an hypothesis that can satisfy the serious student, for it adds nothing to the intelligibility of the original thesis that time is merely extension along a hyper-space. Mr. Dunne's identification of k with c is perhaps the least persuasive aspect of his theory for it requires us to assume that a distance mapped out along a "regressive" time-dimension S_4 or T_1 and a length measured along one of the dimensions of space can be treated as alike in the calculation of velocities, and this assumption is surely at variance with the postulation of a "double character" which *ex hypothesi* is peculiar to time. Mr. Dunne's k is apt to suggest (at any rate to his readers) a homogeneous flow of time recorded by all standard clocks—the very idea the acceptance of which has been rendered extraordinarily difficult by relativity. A candid critic, while he will acknowledge the undeniable merits of Mr. Dunne's book, is likely to carry away the impression that Serialism contains elements of unequal value. It would be hazardous to deny that scientific theories promulgated to-day presuppose a drastic revision of our common-sense notions of time; but it may be doubted whether the suggestion that our tape-measure adroitly used will replace duration can be sustained under a severe intellectual scrutiny.¹

¹ The relativistic world is $(3 + 1)$ dimensional rather than four-dimensional. Prof. R. C. Tolman has recently reminded us that "time should in no sense be considered as the fourth dimension of space, but rather as one, and at that a *unique* one, of the four dimensions of space-time". (*Relativity, Thermodynamics and Cosmology*, p. 29 f.)

(III.)

We can perhaps choose another avenue of approach to the problem of an imaginary time-dimension. The relativity theory, it need hardly be said, deals not with geometry alone but with kinematics. When time is represented as a fourth co-ordinate, the kinematic law of motion of a particle is transformed into the equation of a world-line and physics tends to become "chronogeometry". In appraising the significance of a space-time continuum, certain distinctions ought to be kept well in view. The first distinction that merits notice is the distinction between the space of mathematics and what Professor Rudolf Carnap has termed "the space of experience". The space of mathematics is a geometric-analytic continuum the properties of which are determined by axiom and definition. The "space of experience", while it may be regarded as a three-dimensional euclidean continuum (a special case of general geometric space), contains in our perception of coloured surfaces, planes and solids something which cannot be conveyed by any mathematical definition, however scrupulous, but must actually be sensed. The euclidean representation of the "space of experience" as a three-dimensional continuum is the most convenient one for purposes of physical description and perhaps the most natural. Lest this statement should court misapprehension, we wish to state explicitly that we are not here disputing the assumption that euclidean planimetry obtains only on an infinitely small scale in physical space. Such an assumption, needless to say, can neither be proved nor disproved by an appeal to crude sense-perception or by an appeal to physical investigations conducted in a restricted region of space. To avoid confusion, we shall limit the term "space of experience" to the space in which effective bodily movements are perceived to take place. The suggestion that this space can, for purposes of proximate physical description, be represented as a three-dimensional euclidean continuum need not be set aside. The euclidean representation of the space of perception we shall designate "the space of proximate physical description."

The space of mathematics and "the space of proximate physical description" may both be conveniently distinguished from Minkowski's space-time scheme, which in the present context we may regard as a "topological" space or as a configuration which unifies the chronophysical data furnished by different observers using instruments of optimum accuracy. The possibility of such a unification of chronophysical data is suggested

by the common practice of representing events or occurrences along a time-axis t plotted against one of the space-axes x . It must be distinctly understood, however, that the location of events or occurrences along a geometrically mapped-out time-axis will not, by itself, constitute what we have ventured to call a "topological" configuration. A geometrical representation of space-time, if it is to have a significance for physics, must contain a reference to the chrono-physical data furnished by different sets of recording instruments (*i.e.*, by different observers using "standard" rods and clocks) situated on systems moving with different relative velocities assumed to be uniform in the first instance. In the discussion which follows, a "topological" representation must always be understood as a configuration which, besides fulfilling certain ideal mathematical requirements, welds together experimental data.

The term "experimental datum", if it is not to invite misconception, has to be carefully guarded. The meaning of every *physical magnitude* consists in our attaching certain relevant numbers to physical realities. To take a commonplace and somewhat trivial illustration: given a standard clock ticking out seconds, the *velocity* of a moving body is the number of units of length passed by the body between two successive ticks of the clock. The relevant number we attach to the moving body expresses a relation between a measurable distance and a "time-lapse" registered by a standard clock. Again, when a ray of light passes from one physical medium to another, its direction is changed in such a way that the ratio of the sines of the two angles which the ray makes with the normal to the surface between the media always has the same numerical value. The relevant number we attach to one physical medium against another is the physical constant we call the *refractive index* of the medium. Speaking generally, any physical magnitude in experimental physics must be capable of being defined in terms of the "operations" or methods of measurement adopted. The "operations", needless to add, may be involved (as in the determination of the diameter of a star or of the fine structure constant) and may even be embedded in a complex theory of measurement.

An "experimental datum" in our usage is a number or "isolate" found directly involved in a set of observable physical magnitudes or in a set of measurements. Thus, the velocity of light (especially when it is calculated in the laboratory by using such an apparatus as Foucault's rotating mirror) is a number or "isolate" found directly involved in the measurement of certain physical angles, distances and time-intervals.

Experimental data have not infrequently a profound significance for theoretical physics (as witnessed by the fact that Foucault's measurement of the relative speeds of light in air and in water turned the tide of criticism against the "corpuscular" theory); but it is important to realise in an epistemological discussion that a physical theory need not always be tied up with experimental data. A physical theory may legitimately overstep the limits prescribed by instrumental observation and deal with objects which are "unobservable". Thus, an adequate mathematical description of spectra may be impossible without our postulating the reality (in some sense) of the "spinning electron".

There remain certain conditions which any theory must fulfil if it is at all to be called an unambiguous *physical interpretation* of experimental data. For our present purposes, the specification of one such condition will be sufficient. This condition will be readily understood if we return for a moment to our distinction between the space of mathematics and the space of physical description. A physical length is a quantity which in principle at least is measurable and is ascribable to a real object. This definition of physical length has a content which distinguishes it from the general geometrical definition of length. Geometrically, it is allowable to enlarge a three-dimensional euclidean space into a purely analytic continuum by introducing imaginary points and distances. It is even possible to construct a geometry in which the co-ordinates of points are complex quantities and which yields as a special case the geometry of real points. Such a geometry, however, which deals with complex quantities can have no unambiguous physical significance. It must be remembered—and this is a point which bears closely on the argument presented in this paper—that the position of an imaginary point with respect to a real point on a given base is indeterminate inasmuch as an imaginary length $\sqrt{-1}$ and a real length $\sqrt{+1}$ are "incommensurable" and do not involve in themselves any inherent relation as to magnitude.¹ A length $\sqrt{-1}$ cannot even in principle be treated as a physical magnitude ascribable to any real object (whether defined to be "observable" or "unobservable"), for such a length can bear no conceivable relation to the laboratory operations which are involved in the measurement of physical length.²

¹ C. L. S. Hatton, *The Theory of the Imaginary in Geometry* (Cambridge), ch. i.

² The $\sqrt{-1}$ which is the integral unit of imaginary geometry must in an epistemological context be distinguished from the "operator" i or j which has played the part of a useful auxiliary in physical description.

A fortiori, a velocity which is equal to $\sqrt{-1}$ can have no physical significance; for velocity in all physical description is a number or "isolate" expressing a relation between lengths and time-intervals which, by very definition, are physical magnitudes. Without pursuing the subject of scientific measurement further, we may content ourselves with the remark that an imaginary velocity i cannot be an experimental datum in the sense defined above, and that no mathematical representation in which such a velocity is definitely implied can be treated even in principle as referring to a physically determinate reality.

(IV.)

We shall now explore the epistemological consequences which follow from a geometrical representation of the space-time continuum as an euclidean space of two dimensions analogous to that of Minkowski. Let us assume that in this representation, ot , ox , are the time and space axes of an arbitrarily selected "stationary" observer S , the point O representing a definite event in the world-line ot which is the locus of all events in S 's career. If A_t be one of these events, the vector OA_t may be taken as a measure of the duration which will be indicated as a "time-lapse" in the standard clock which S carries. S from his own point of view is "stationary"; he will regard his movement as in time only. Spatial separation he will represent as a displacement along the line ox perpendicular to ot . (His "space of proximate physical description" is a line along which his own displacement has no component whatsoever.)

Let us suppose that the event O in our mathematical representation is an event of coincidence, in space, of our "stationary" observer S with a second observer S' moving with uniform relative velocity v with respect to S . Let us further suppose that the event O is coincident with the flashing of a beam of light by S . At time t as reckoned by the standard clock which S carries, the displacement of S' parallel to the space-axis of S will be vt . Hence, S will represent the track of S' in space-time by the straight line $x = vt$ and he will be disposed to treat this line as the time-axis of S' and the perpendicular to it through O as the space-axis of S' . Let us denote these lines by ot' and ox' respectively.

It is evident that at time t , S will represent the track of any ray of light belonging to the beam of light flashed at O by the straight line $x = ct$ making an angle $\tan^{-1}c$ with his world-line. Let us call this line $O\Omega$. Regarding the same track from the

standpoint of S' (an observer who from his own standpoint is using standard rods and clocks), S will have to concede that it can be represented by a line making an angle $\tan^{-1}c$ with what he conceives to be the world-line of S'. The Principle of the Constancy of the Velocity of Light, therefore, when fitted into our geometrical representation of space-time, carries with it the following consequence: the light-track $O\Omega$ makes the same angle $\tan^{-1}c$ with both ot' and ot , the world-lines of S' and S respectively. This remark is true whatever be the velocity of S' relative to S. That is to say, $O\Omega$ is a straight line which makes the same (constant) angle with the world-line of S' whatever be the inclination of this world-line to the world-line of S. Thus the constancy of the velocity of light leads to the isotropism of $O\Omega$ with respect to all straight lines in our mathematical representation. More explicitly, the only possible representation of the light-track in a geometrical configuration which has reference to the experimental data furnished by different observers (each observer using what he takes to be rods and clocks of standard construction) is by one of the isotropic (or circular) lines $x = \pm it$ through the point O which represents an event of coincidence.

This brings us to the crux we must visualise clearly. The straight lines $x = \pm it$ are not mathematically real in our euclidean representation. If we wish to relate our ideal euclidean scheme to the instrumental data furnished by observers using standard rods and clocks (*i.e.*, if the geometrical representation is to have a significance for physics), we must identify one of the imaginary lines $x = \pm it$ with the line $x = ct$; that is, we have to change t into $\pm \frac{c}{i}t$. Now i is a constant which deter-

mines the metric of the mathematical continuum by means of which we have chosen to represent space-time.¹ c , on the other hand, is a kinematic constant, depending as it does on the units of space and time employed in experiment. Hence, in linking c with the i of our mathematical representation, we are fitting an experimental datum into a field or continuum which cannot even in principle be defined as a *physically* determinate reality.²

¹ This point can perhaps be more readily grasped when we remember that euclidean geometry is identical with non-euclidean geometry on a sphere of radius $i (= \sqrt{-1})$. Cf. H. Weyl, *Space-Time-Matter*, p. 83 f.

² An essential feature of the field in question is that its metric is determined with respect to two *imaginary* points. In mathematical language, the Cayley-Klein measure-determination of the continuum is fixed with reference to an Absolute Conic formed by these points. (Cf. J. L. Coolidge, *Non-Euclidean Geometry*, p. 88 f.) It is by integrally relating

We can easily satisfy ourselves that the ideas sketched in the preceding paragraph hold the key to all transformations in Minkowski's "topological" (i.e., space-time) scheme. In our mathematical plane, the co-ordinates of an event defined with respect to the frame of reference of our "stationary" observer S and with respect to what S conceives to be the frame of reference of S' are connected by the relations

$$\left. \begin{aligned} x' &= x \cos \theta - t \sin \theta \\ t' &= x \sin \theta + t \cos \theta \end{aligned} \right\} \text{where } \theta = \tan^{-1} v. \quad (i)$$

These relations, as must be evident, are grounded in the conventional representation of the world-line of S' by S on the basis of measurements of relative velocity. To make these relations tally with the paradoxical behaviour of light, we have to change

$$t \text{ into } \pm \frac{c}{i} t \text{ and } v = \frac{x_{s'}}{t_{s'}} \text{ into } \frac{x_{s'}}{\pm \frac{c}{i} t_{s'}} = \pm \frac{i}{c} v.^1$$

Changing t and t' thus in (i), we get

$$\left. \begin{aligned} x' &= \beta (x - vt) \\ t' &= \beta \left(t - \frac{vx}{c^2} \right) \end{aligned} \right\} \text{where } \beta = \frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}. \quad (ii)$$

We see that the representation of the Lorentz transformation-formulae as a rotation of axes through an imaginary angle in Minkowski's euclidean space-time scheme is not just a useful way of picturing algebraic operations involving only real numbers but is a necessary geometrical consequence of our having identified the trajectory of light with one of the imaginary lines $x = \pm it$ through O.

Einstein's law of composition of velocities can be consistently deduced from our premisses. If v_2 is the velocity of an observer S_2 relative to an observer S, and v_1 the velocity of an observer S_1 relative to S, the world-lines of S_2 and S_1 will be represented

the light-tracks to these imaginary points that we get Minkowski's geometrical scheme. (Vide, proofs of the transformation-formulae of the special theory of relativity furnished in the succeeding paragraphs.) Minkowski's "topological" scheme contains, therefore, an essential feature of a geometrical representation in which the direction of the world-line of light, and hence the velocity of light, can have no physical significance.

¹ Cf. The note on "The rôle of i in the special theory of relativity" contributed to *The Mathematical Gazette* (July, 1936) by C. T. Rajagopal and C. T. Krishnama Chari.

by S as $x = v_2 t$ and $x = v_1 t$ respectively. Hence, in a conventional mathematical representation which has reference only to material velocities, the world-line of S_2 will be represented by S_1 as $x = \frac{v_2 - v_1}{1 + v_1 v_2} t$. If, however, we fit the paradoxical behaviour of light into this conventional geometrical scheme, we have to change t into $\pm \frac{c}{i} t$ and v into $\pm \frac{i}{c} v$. Changing t and v thus, we get the equation to the world-line of S_2 as represented by S_1 in the form $x = \frac{v_2 - v_1}{1 - \frac{v_2 v_1}{c^2}} t$. It follows that the velocity

of S_2 relative to S_1 (regarded as "stationary") is $\frac{v_2 - v_1}{1 - \frac{v_2 v_1}{c^2}}$.

We also see that in our mathematical representation the locus of events on the world-lines of observers separated from an event of coincidence by equal time-intervals ($= T$) defined with respect to the standard clocks carried by the observers is the circle

$$x^2 + t^2 = T^2 \quad . \quad . \quad . \quad (iii)$$

In this representation, "simultaneity" can be defined (at any rate conventionally) with respect to standard clocks; for the invariant $x^2 + t^2$ is the square of a time-lapse (T) the intrinsic magnitude of which is not in any way affected by our choice of a frame of reference.

If, now, in (iii), we substitute $\pm \frac{c}{i} t$ for t and $\pm \frac{c}{i} T$ for T , we get

$$\begin{aligned} x^2 - c^2 t^2 &= -c^2 T^2 \\ \text{or, } c^2 t^2 - x^2 &= c^2 T^2 \quad . \quad . \quad . \quad (iv) \end{aligned}$$

This is the equation of Minkowski's hyperbola. The hyperbola is the locus of events on the world-lines of observers separated from an event of coincidence by "equal space-time intervals". In this representation, lengths and time-intervals cannot be defined in a "dyadic" fashion (*i.e.*, with respect to all the frames of reference). The slope of the tangent at any point on the hyperbola determined with respect to the world-line of a "stationary" observer can be taken as a measure of the "distortion" of the co-ordinates of any moving observer as these are viewed by the "stationary" observer. The "distortion" can have no unambiguous physical significance, for it cannot be uniquely

ascribed to the same set of objects by all observers (*i.e.*, no measuring rod or clock in this representation can be treated as "neutral" with respect to the system it measures). As our choice of a "stationary" observer is perfectly arbitrary, we have to postulate that every observer is using what *from his own point of view* are rods and clocks of standard construction but what *from the point of view of an observer on any other system* are not rods and clocks of standard construction. It will also be noticed that the light-tracks in this representation of the space-time continuum are the asymptotes to the hyperbola. We can infer from this that the velocity of light for any observer is the measurable and yet unattainable upper bound of the velocity of any material body.

The line of approach we have adopted here shows that the special theory of relativity in effect resolves the paradoxes about lengths and time-intervals by fusing the kinematic constant c with a continuum which cannot be treated as a physically determinate reality. Thus the fact that space-measures and time-measures have no unambiguous significance on Minkowski's hyperbola becomes perfectly intelligible when we remember that such a representation is specifically grounded in the assumption that a line which on the basis of experimental data is $x = ct$ can be identified with one of the imaginary lines $x = \pm it$. The fact that the velocity of light has the rôle of a limiting velocity in Minkowski's geometry is merely a reflection of an important feature of our ideal euclidean representation, namely, that the lines $x = \pm it$ are the asymptotes to any circle with centre at O .¹ Again, the fact that the space-time separation of any two

¹ Einstein's law of composition of velocities can be represented on a Lobatchevskian plane. The proof may be furnished here as it has an epistemological interest.

Let OX, OY , be the rectangular axes in a Lobatchevskian plane the "space constant" of which is c . We may specify a point on OX by the single co-ordinate X or by a pair of homogeneous co-ordinates: $X = c \sinh \frac{X}{c}$, $t = \cosh \frac{X}{c}$. (D. M. Y. Sommerville, *Non-Euclidean Geometry*, p. 127, § 2.) The difference X between the co-ordinates X_1, X_2 , of two points on OX (*i.e.*, the distance between the points) is given by

$$X = X_2 - X_1$$

(Sommerville, p. 129, § 4). The relation can be expressed in terms of homogeneous co-ordinates thus:

$$\frac{x}{ct} = \frac{\frac{x_2}{ct_2} - \frac{x_1}{ct_1}}{1 - \frac{x_1 x_2}{c^2 t_1 t_2}}$$

events along the light-track is zero in the relativistic scheme is accounted for by the theorem that the distance between any two points on a circular line is zero.¹

or, replacing $\frac{x}{t}$, $\frac{x_1}{t_1}$, $\frac{x_2}{t_2}$, by v , v_1 , v_2 , respectively,

$$v = \frac{v_2 - v_1}{1 - v_1 v_2 / c^2}$$

which is Einstein's law of composition of velocities. We see that in this scheme material velocities defined in terms of x and t by the relation $v = x/t$ are represented by points on OX; the formula for the difference between two such velocities is simply the formula for the difference between the (rectangular) co-ordinates of the points. But it should be noticed at the same time that the velocity of light which in its fundamental aspect is a constant associated with the Lobatchevskian plane can be represented on OX only by one of the points at infinity. The epistemological paradox, therefore, reappears. The finite constant velocity of light, which is an experimental datum for physics, can, in the mathematical representation, have no unambiguous physical significance. (The relation between the homogeneous co-ordinates used is $x^2 - c^2 t^2 = -c^2$. This becomes identical with the relation between the co-ordinates of events on Minkowski's hyperbola if we use the homogeneous co-ordinates:

$$x = Tc \sinh \frac{X}{c}, t = T \cosh \frac{X}{c}, \text{ where } T \text{ is the value of } t \text{ when } X = 0.)$$

The last remark applies *mutatis mutandis* to the representation of Einstein's law of composition of velocities on a sphere of imaginary radius ci . (Cf. L. Silberstein, *The Theory of Relativity* (1914), footnote, p. 176.)

¹ In the above discussion we have not touched on the "wave character" of light. But it can be shown that, if this "wave character" is mathematically formulated in a way compatible with the demands of the special theory of relativity, a difficulty is bound to present itself.

For instance, assuming the equation of a system of waves with wave number κ and frequency ν in the form

$$\psi = \psi_0 \cos 2\pi (\kappa x - \nu t)$$

we see that when $\frac{x}{t} = c$ (as in the case of light),

$$\psi = \psi_0 \text{ since } \kappa = \nu x / c^2 t = \nu / c,$$

x being equal to ct .*

Our mathematical representation of the light-wave is independent of x and t (conventionally defined space and time co-ordinates). This is simply a consequence of our carrying over to the wave theory the paradox that the space-time separation of any two events along the light-track is zero.

It is the merest truism to-day that a mechanical theory of the propagation of light is neither possible nor necessary. But the emphasis usually laid on the question whether c is a constant associated with a luminiferous ether obscures the epistemological issue—the nature of the geometrical continuum with which the kinematic constant is fused. Prof. Hans Reichenbach lays his finger on the real difficulty when he says that "the measurement of a velocity does not signify the ascertainment of

(V.)

Space-time has been construed as an objectively real four-dimensional Universe which observers divide up into a multiplicity of spaces and times. The serious difficulty involved in the very idea of a metrical space-time has seldom been acknowledged. The trajectory of light which in our two-dimensional space-scheme ought to be represented by one of the circular lines $x = \pm it$ is represented by observers using the conventional unit of time (the second) as $x = ct$. In relativistic physics, the unit of time is so chosen that i enters into the description of the world-line of light and into all transformation-formulæ.

Changing t into $\pm \frac{c}{i}t$ is an operation which may appear mathematically unimportant and even trivial. Its epistemological significance, nevertheless, is fundamental; for the configuration from which we pass (representing a plane on which time is reckoned in terms of a conventional physical unit) is not, epistemologically speaking, the configuration to which we pass which preserves the isotropism of light-velocity. On the latter plane—the “topological” plane to which relativistic calculations refer—the time-co-ordinate is found to involve both c and i .

The adoption of the view that the relativistic i is introduced merely for the sake of “formal symmetry” will not help us in this predicament because even on this “formalist” view we have to concede that the employment of a conventional unit of time (a “clock-interval”) is misleading as it does not do justice to the paradoxical behaviour of light which at any rate must be treated as a fact of experience. A measurable velocity the magnitude of which remains identical with respect to all frames of reference renders it impossible for us to rest content with the view that observable lengths and time-intervals are unambiguously ascribable to nature. It follows from this that the velocity c itself, depending as it does on units of space and time, cannot be unambiguously ascribed to light. It cannot be said that c is a physical constant, for it cannot be defined in

independent data, but the erection of a structure into that fluid of the electric field which is, of itself, formless”. (*Atom and Cosmos*, p. 136).

* The equation for a system of waves is adapted from J. Frenkel's *Wave Mechanics*, Elementary theory (Oxford), 1, § 4. The relation $\kappa = \frac{vx}{c^2t}$ is

a consequence of the fact that $\kappa x - vt$ behaves like $x^2 - c^2t^2$ leading to the equation $\kappa x/x^2 = vt/c^2t^2$. The three spatial dimensions of Frenkel have been replaced, for the sake of simplicity, by a single dimension in our mathematical representation.

terms of a *unique* set of physical operations ; nor can it be unambiguously affirmed that c remains constant because the units of space and time vary as we pass from one frame of reference to another, for the alterations in rods and clocks cannot be unambiguously interpreted. Every moving observer is, from his own point of view, "stationary" (*i.e.*, is using standard rods and clocks). The formal representation of the transformation-formulæ of the special theory of relativity by hyperbolic functions will not abolish the epistemological problem suggested by the Michelson-Morley experiment. *The unit of time employed in experiment cloaks the essential characteristic of the phenomenon of light propagation.* The "respectivity" of space-measures and time-measures follows from and is rooted in the isotropism of light velocity ; the space-time invariant is a metrical consequence of our having fused an experimental datum—the finite constant velocity of light—with a geometrical continuum into which i enters integrally. The dilemma for an epistemologist is inescapable. The phenomenalist view leaves the "respectivity" of physical magnitudes quite unintelligible ; the assumption, on the other hand, that the "space-time separation" of events can be treated as a physical characteristic of a four-dimensional continuum ushers in questions of a most disquieting sort. It is the very meaning of "physical reality" which now needs to be defined.¹

We wish to stress here what we conceive to be the inadequacy of all "mechanico-morphic"² representations of concrete becoming in nature. Duration or succession as it presents itself to our consciousness is something more than mere "extension" in a time-dimension, for it involves an asymmetrical relation of no-longer-and-now, a relation of uni-directional sequence as

¹ It is impossible within the limits of this paper to discuss the significance of the space-time scheme which is developed in the general theory of relativity by the adoption of non-euclidean metrics. If we accept Einstein's Principle of Equivalence, the result that the world-line of light is a geodesic of zero length must be true in any system of co-ordinates and must be true in a gravitational field. The Principle of the Constancy of the Velocity of Light is retained in the general theory of relativity though in a form different from that which underlies the special theory. (H. Weyl, *Space-Time-Matter*, Appendix I ; T. P. Nunn, *Relativity and Gravitation*, footnote, p. 107). The adoption of non-euclidean metrics for a gravitational field can hardly be regarded as the solution of the epistemological problem with which we are here concerned.

² The term "mechanico-morphic" is used by Mr. J. Murphy in his biographical introduction to Prof. Erwin Schrödinger's *Science and the Human Temperament* (p. 19). A "mechanico-morphic" representation in the present context means a conventional geometrical-physical representation.

one might call it. Physical description, while it contains "time-intervals", does not contain duration in the unique sense. This is due to the fact that physical description employs a unit which, even when it is referred to an astronomical phenomenon such as the rotation of the earth upon its axis, remains conventional. The framework of years, days and hours, indispensable as it is in a physical description of events—especially in a physical description of remote events—can yet never be anything more than a conceptual organisation of our experience of concrete change. A metaphysical inquiry into the nature of time will be abortive if it ignores the epistemological distinction between our conscious experience of duration and the physical description of time. The "direction of time" which is so significant an aspect of experienced duration and which enters into every consciously experienced "now" can only be conventionally defined with respect to a clock ticking out seconds. We may agree, *faute de mieux*, to regard as the time-direction that in which the number of beats registered by the clock is increasing. The conventional, physical unit does not embody the most characteristic feature of the duration or the process of which we are aware—its uni-directional character.

It must be acknowledged at the same time that our conscious experience of duration is mediated by complex factors (such as interest and expectancy) which militate against the adoption of purely psychological standards in experimental physics. Scientific description tends to emphasise "clock-time" and "clock-intervals" more than psychologically experienced duration. The objectivity of the scientific description cannot in principle be disputed so long as a conventional definition of "past", "present" and "future" with respect to standard clocks is held to be possible. Common sense takes it for granted that such a conventional definition of time is possible because it finds it convenient in practice to do so. It is convenient in practice to assume that "equal time-intervals" registered by standard clocks express an intrinsically identical stretch of duration.

The theory of relativity casts doubt on the objectivity of descriptions of natural events which rest only on "clock-time" and "clock-intervals". The *contretemps* in science is rather peculiar, and in this paper we have attempted to elucidate the origin and the significance of the relativistic anomalies. The assumption that natural events can be represented along a time-axis plotted against a space-axis leads to our envisaging the possibility that a geometric representation of the space-time continuum ought to be possible. In such a representation the kinematic law of motion of a particle is expressed as the equation

of a world-line ; but the direction of the world-line—the direction of the time-axis for any particle—is conventionally fixed on the basis of the data furnished by experiment (*i.e.*, on the basis of velocity measurements). This conventional description breaks down when the paradoxical behaviour of light is fitted into the mathematical scheme. In the ideal euclidean plane, the trajectory of light can be represented only as one of a pair of circular lines and with respect to such a line the orientation of the world-line of a particle—the direction of the time-axis for a particle—cannot be conventionally determined. Thus the purely geometrical representation of the trajectory of light imposes a demand on experimental physics which experimental physics is unable to meet. A “topological” representation fulfils the mathematical demand, but conditionally, without discarding experimental data. The latter representation permits a fairly satisfactory formulation of certain metrical relations in accordance with the demand for “invariance”. The time-dimension, however, in a “topological” configuration presents a serious problem. In such a representation, “before” and “after” cannot be defined unambiguously with respect to standard clocks ticking out seconds. We have seen that this is merely a consequence of the fact that the *i* which enters *explicitly* into the ideal euclidean plane is *implied* in all relational formulæ in a “topological” representation.

In philosophical discussions concerning relativity, it has been customary to assume that Einstein's theory departs from the traditional “Absolute” and “Relational” theories of time only by making the choice of a “time-interval”, or the choice of a “time-relation” between events, arbitrary. This general statement of the problem of relativity leaves untouched the crucial difficulty involved in relativistic descriptions of time which we have emphasised in the foregoing paragraph. The relativity theory is not merely concerned with the problem of measurement ; it is primarily concerned with the failure of conventional descriptions of time-flow. The paradoxes about space-measures and time-measures which to an experimentalist must always appear elusive and baffling the relativist resolves by constructing a world-geometry. Our own line of approach to the problem does not warrant our ascribing any unambiguous “reality” to the world-geometry. Such a geometry is at best a mathematical expedient devised to get over the failure of conventional space-time descriptions. The failure of conventional space-time descriptions suggests that the durational sequence of events, in other words, the passage from the “present” to the “future” which is of the very essence of any concrete instance of

becoming in nature, may be a kind of fact which cannot ultimately be discussed in terms of physical measurement alone.¹

An epistemological analysis of our perception of time is highly pertinent to the situation created by relativity. Our conscious experience of change, as already pointed out, includes besides a reference to "time-intervals" an awareness of uni-directional sequence. The psychological factors which mediate this awareness of duration may not be easy to handle from the standpoint of an experimentalist but they do not render unveridical our awareness of what must be regarded as the most essential characteristic of time. In an epistemological inquiry we must not confuse between our *awareness of duration* and *the duration* of which we are aware.² Bergsonian theories of time which equate our awareness of duration with the duration or the reality itself involve this confusion between the noetic and the content of the noetic. Such theories can provide no satisfactory explanation of illusory and abnormal perceptions of time-flow. For our own part, we would emphasise not the psychological conditions which mediate our awareness of duration but the duration or the process which is a primary datum for consciousness. Duration or succession as it enters into our experience of reality presents a feature of which a physical representation seems impossible. It is in this context that an epistemologist will endeavour to understand the relativistic paradox that physical measurement alone does not provide for an unambiguous definition of any "now" in nature. Real duration—concrete becoming in nature—may well be an aspect of the Universe which cannot be defined exclusively in terms of physical magnitudes. While such an assumption remains metaphysical and must derive its force from arguments other than those adduced merely from mathematical physics, it cannot be too carefully borne in mind that there is nothing in Minkowski's "topological" scheme which renders the assumption itself either unmeaning or unnecessary. On the contrary, the "topological" scheme suggests that a purely metrical theory of duration must inevitably be exposed to certain disabling restrictions. The adoption of the epistemological view we have developed here would render equivocal and dubious theories which attempt to explain percipience by housing percipients in a metrical Universe of space-time.

¹ Our approach to the problem of space-time invites comparison with the line of development followed by Prof. F. A. Lindemann in his book, *The Physical Significance of the Quantum Theory* (Oxford), cf. esp. xiv and xv.

² It is the merit of Prof. J. A. Gunn to have emphasised in recent years this distinction. Cf. *The Journal of Philosophical Studies*, vol. IV., No. 14, p. 187.

III.—THE UNITY OF THE BERKELEIAN PHILOSOPHY (II.).

BY A. A. LUCE.

THE second part of my thesis consists in the following Analysis (a 'harmony' may I call it?) of the doctrinal contents of Berkeley's works. The first part has proved, I think, that Berkeley was not aware of any change in his views; the second part answers the question: Did he change without knowing it?

The Analysis speaks for itself; but I will say a few words to introduce it and to summarize its results. In it we deal with the internal evidence for the unity of the Berkeleyian philosophy. It is a little awkward to have to speak of 'internal evidence' for a unity I have called external; but I expect my meaning is clear. I simply want to show that the doctrines of the *Principles* coincide with the doctrines of the later works, and I am not now touching on the question of the internal consistency of the *Principles* itself. Are the divine and human elements in the *esse percipi* reconcilable? Is the causal action of the infinite Spirit consistent with that of finite spirits? Such questions I leave open, and keep to the humbler task of taking the doctrines of the *Principles* as they stand, and finding out whether they square with what Berkeley taught in later life.

I read the books in sequence and listed the topics as they presented themselves. I then chose from the list eighteen doctrines that seem to me to be of primary importance; with hardly one exception I found them treated, or incidentally mentioned or assumed, in a wide range of works from the *Principles* to the *Siris*; upon that fact the weight of the case rests. The remaining topics, twenty-six in number, which I have styled 'subsidiary doctrines and characteristic details of arguments', are not to be neglected because they are put into a second-class compartment; the distinction is only *ad hoc*, and the classification arbitrary. In their own way they constitute a strong support for my thesis.

'What of the negative instance?' I may be asked. 'Are there no doctrines that upset your contention? Are there no

inconsistencies between the earlier and the later books? Are there no doctrines in the *Siris* that contradict the *Principles*? I can only reply that I have done my best to do the work fairly. It is a harder piece of work than it looks, and I do not claim to have produced a complete concordance. There may be oversights in it; but I have not consciously dodged difficulties or omitted the negative instance. The three chief difficulties are dealt with in Notes A, B, and C at the end of this paper. Here I will mention a few smaller points. Some think that Berkeley varied his doctrine of abstraction, and that the 1734 addition of 'So far he may abstract' to Prin., Introd. 16 is a climb-down; but that addition is no more than he said from the start; he always maintained that there is a right as well as a wrong abstraction (see § 10). Two inconsistencies, which, to me, are merely verbal must be admitted. Unthinking second causes are nominally accepted in *Siris*, 160; but Berkeley makes it clear that they are not agents, *i.e.*, they are signs, not efficient causes. Again, in *Siris*, 266, the Pythagoreans and Platonists receive creditable mention for distinguishing primary qualities from secondary; perhaps he is only trying to show that the ancients were *au fait* with the permanent problems of perception; perhaps he just accepts the distinction, as we all do, to the extent of regarding it as a convenient grouping of qualities; but the rest of the section shows that he could not possibly be taken as approving the Lockian doctrine of primary qualities as efficient causes of the secondary.

It may appear a hard thing to credit a genius like Berkeley with the authorship of a static, undeveloping system, but that is due to our twentieth-century point of view. When we stand back from the tedious details, and see Berkeley as a real man in his own times, what else could we expect from him? 'Development' was not known then, much less prized. Berkeley sprang up in that happy period between the old and the new scholasticism, when young men sought truth, not learned it. Years of thought went to the making of the *Principles*. Immaterialism was to him a hypothesis that became a creed. His philosophical creed entered the fabric of his mind, colouring his religion and his whole outlook on life. Besides, externally it had a very fair measure of success; he was called by Swift 'the founder of a sect called the Immaterialists'; most of his books went through more than one edition; he was famous in Ireland; he was known and discussed and written about in England; he made converts in Scotland; on the Continent books of his were reviewed and translated; he himself had

planted his philosophy in the New World. His thought found a footing in Yale and Harvard.

Dr. Johnson, the first President of King's College (afterwards Columbia University), dedicated to him a manual of philosophy, of which the main aim was, as he writes to Berkeley in the year of its publication (1752), to draw 'attention to your Lordship's most excellent writings'.¹ In a word, Berkeley's philosophy, his one and only philosophy, attained a recognized position in his lifetime, and was sedulously cherished and zealously defended by its author. With these facts in mind we shall not be unprepared for the result of the forthcoming Analysis. In summary form it is this : through a series of books, written over a period of thirty-five years with various aims and motives, one single thread runs ; and that thread is the philosophy of Berkeley's early years, the philosophy of the *Principles*, his title to fame, the philosophy from which he never departed and which he never changed.

DOCTRINAL ANALYSIS OF BERKELEY'S PHILOSOPHICAL WORKS.

Arranged under two heads : doctrines of primary importance ; subsidiary doctrines and characteristic details of arguments.

Note.—An asterisk after a number denotes that the passage, in the writer's opinion, contains the doctrine in question by implication, not explicitly.

The following abbreviations are used :

Vision	<i>An Essay towards a New Theory of Vision.</i>
Prin.	<i>A Treatise concerning the Principles of Human Knowledge.</i>
Hylas	<i>Three Dialogues between Hylas and Philonous.</i>
Motu	<i>De Motu.</i>
Alc.	<i>Alciphron or the Minute Philosopher.</i>
Vindict.	<i>The Theory of Vision . . . Vindicated and Explained.</i>
Anal.	<i>The Analyst.</i>
Defence	<i>A Defence of Free-Thinking in Mathematics.</i>

The Three Dialogues, unlike the remainder of Berkeley's works here examined, has no numbered sections ; in references to it I give the page number in Fraser's (1901) edition of the *Works*, Vol. 1. The section numbers in the *Alciphron* are those given in the same edition.

¹ Wild, *George Berkeley*, p. 312, n.

Doctrines of Primary Importance.

There is no matter (or absolute existence of objects of sense): Prin., 3, *et passim*. Hylas, 381, *et passim*. Motu, 21,* 24.* Alc., IV., 8-15.* Vindic., 12. Anal. Queries, 7, 20. Siris, 155, 251, 266, 270-271, 311, 317-319.

There are no abstract general ideas (abbrev. abstract ideas): Vision, 122-125. Prin., Introd., *passim*. Prin., 11, 81, 97-100, 118, 143. Hylas, Pref., 402-404, 427 (3rd ed.), 436, 467. Motu, 7, 8, 66, *et passim*. Alc., VII. (all editions), *passim*. Anal. Queries, 7, 8, 20. Defence, 20, 45-48. Siris, 323, 335.

All objects of sense are 'in the mind' (or, are ideas): Vision, 43, *et passim* (of objects of sight only). Prin., 1, 2, *et passim*. Hylas, 383, *et passim*. Motu, 21, 63.* Alc., VII., 5. Vindic., 11, 29, 39. Siris ('appearances', see note A below, p. 187), 251, 264, 266, 270, 285, 292, 308, 310, 335.

Objects of sense may be outside *my* mind (independent, external, perceivable, imprinted, sensible, permanent): Prin., 1, 8, 56, 74, 90, 109, 145, 148. Hylas, 422-423, 427-428, 446-447, 451, 470, 484. Vindic., 13. Siris, 266.

All objects of sense are passive (inactive, impotent, effects): Prin., 25, 61, 64-66, 141. Hylas, 426, 430, 447. Motu, 22. Alc., VII., 5. Vindic., 11. Siris, 153, 220-221, 245-250, 257, 291-292, 308, 335.

There are two heads of human knowledge, of ideas (or, bodies, or, unthinking things) and of spirits: Prin., 86, 89 (adds 'relations' in 2nd ed., see note C below, p. 189). Hylas, 437, 453. Motu, 21. Alc., VII., 5.* Siris, 290, 292-293, 296.

Spirits are the only true causes: Prin., 26, 32, 102, 146. Hylas, 429-434, 453-454, 479. Motu, 69-72. Vindic., 11. Siris, 154-155, 231, 254, 285, ct. 160, and see my note above, p. 181.

The objects of sense form the universal language (arbitrary, ordered symbols) by which God speaks to men: Vision, 140-148 (of objects of vision only). Prin., 44, 65-66, 108. Alc., IV., 5-15. Vindic., 40. Siris, 173, 252-254.

Spirits may be known, though not as ideas are known. The self is known immediately by consciousness or reflection, other spirits mediately by their effects, God by an act of reason, finite spirits by analogy: Prin., 89 (especially the addition in the 2nd ed.), 135-149. Hylas, 447-449, 449-451 (added 3rd ed.). Motu, 21. Alc., IV., 5; VII., 5. Siris, 290, 297, 303.

The changes in the objects of sense, uncaused by us, prove

the existence of God : Prin., 29-33, 146. Hylas, 424-425, 427-428. Motu, 69. Alc., IV., 14. Vindic., 13. Siris, 237.

God is intimately present and immediately operative in the world of sense and finite spirit : Prin., 54, 60-63, 66, 146-156. Hylas, 427-428, 447, 453, 474. Motu, 69 (*continet*). Alc., IV., 14. Vindic., title-page, 8. Siris, 237, *et passim*.

All things depend on God : Prin., 6, 149, *et passim*. Hylas, 432, *et passim*. Motu, 34. Alc., IV., 14, *et passim*. Siris, 303, *et passim*.

The principal human faculties of knowledge are sense and reason : Vision, 3, 126, *et passim* (sight, touch, etc., understanding, intellect, judgement, inference). Prin., Introd., 1 ; Prin., 18, 27 (understanding as a primary function of spirit), 72, 75, 82, 123, 148-149. Hylas, 383, 404 ('pure intellect', see note B below, p. 188), 415-416, 448, 465, 476. Motu, 21, 53. Alc., VI., 17-20 ; VII., 11-14. Vindic., 20, 42-43. Siris, 253, 264, 266, 296, 303-305.

The imagination (or fancy, suggestion, experience, memory) is a link between sense and reason : Vision, 3, 66, 123, 126, 154. Prin., Introd., 10 ; Prin., 1, 23, 28, 30, 33, 36-37, 43, 75. Hylas, 404, 413-416, 422, 428, 465-466, 476. Motu, 53. Alc., VII., 11-14. Vindic., 42-43, 50-52. Siris, 264, 296, 303-305.

The senses are trustworthy : Prin., 40, 101. Hylas, 427 (3rd ed.), 445-446, 454-455, 463, 482. Alc., IV., 9 ; VII., 24.* Vindic., 20. Siris, 292, 303-305 (see note B below, p. 188).

Our knowledge of nature is predictive and practical : Vision, 147-148. Prin., 31, 58-59. Hylas, Pref. Alc., IV., 12-15 ; VII., 11-15. Vindic., 36. Siris, 160, 234, 252-253.

The connection between the objects of sense is customary (by experience, arbitrary), not necessary : Vision, 5, 17, 23, 62-66, 72, 102-105. Prin., 30-31, 106. Hylas, 383, 460-461. Alc., IV., 11-12. Vindic., 14, 29-30, 39-43. Siris, 239-54.

The laws of nature are formulations of the general rules of God's habitual working (are constituted by the will of God) : Prin., 30-32, 57, 62, 105-109, 146, 151. Hylas, 433, 447, 480. Motu, 37 (*cf.* 69). Alc., IV., 11-12.* Vindic., 67. Siris, 160-161, 231-234, 243, 261, 293.

Subsidiary Doctrines and Characteristic Details of Arguments

An active being is unlike an idea : Prin., 8, 25, 27. Hylas, 447-448. Alc., VII., 5. Vindic., 11.

A spirit is perceived only by its effects : Prin., 27, 147. Hylas, 447-448. Motu, 10 (of force). Alc., IV., 4. Siris, 234 (of force).

God is incorporeal : Prin., 57. Hylas, 458-459. Motu, 69.*
Anal. Queries, 14. Siris, 289, 319.

God is wise, powerful, and good : Prin., 32, 57, 72, 146, 151.
Hylas, 428. Motu, 34. Alc., IV., 17-22.

Ideas in the mind of God may be admitted, but with reserve
and discrimination : Prin., 70-76. Hylas, 452, 459, 468. Siris,
337 (see note A below, p. 186).

The term 'idea' may be used in philosophy to express the
true nature of the unthinking thing : Prin., 38-39, 90. Hylas,
427-428, 453, 463, 471, 484. Alc., VII., 5. Vindic., 11.

Things are 'in the mind' by way of idea, not mode : Prin.,
49, 142. Hylas, 455, 470. Siris, 266.*

Sensations are perfectly known, there being nothing in them
that is not perceived : Prin., 87, 101. Hylas, 418. Siris, 292.

There are perceivable ideas (possible ideas, sensibilia) : Prin.,
8, 78. Hylas, 446, 451.

I make some ideas (am truly active) : Prin., 28. Hylas, 428.
Motu, 30. Siris, 257.

There are differences both in originating cause and in psycholo-
gical character, between ideas of sense and ideas of the imagina-
tion : Prin., 29-30, 33. Hylas, 428, 452, 480. Siris, 264.

Objects of sense, by contrast with objects of the imagination,
constitute an order of reality (less often, of greater reality) :
Prin., 33-36, 82, 88. Hylas, 417-418, 422-424, 428, 452, 455, 469.
Vindic., 20. Siris, 335.

The connection between ideas is that of sign and thing signified,
not that of cause and effect : Prin. 64-65, 108. Hylas, 429-430.
Motu, 69-71. Alc., IV., 7; VII., 11. Vindic., 13. Siris,
258, 261, 266.

All ideas are particulars : Prin., Introd., 24; Prin., 12.*
Hylas, 403. Motu, 4, 7. Alc., VII., 11, 12 (3rd ed.). Defence,
47. Siris (things many, not one), 347, 355.

Ideas become general by being made to represent other par-
ticular ideas : Prin., Introd., 12, 15. Alc., VII., 11. Anal.
Queries, 17-18. Defence, 45-48.

There is no difference in kind between primary and secondary
qualities. The primary qualities are not causes of the secondary :
Prin., 9-10, 14-15, 73. Hylas, 397-405. Motu, 22 ct. Siris,
266, and see note above, p. 181.

The appeal to average experience confirms his views : Vision,
12. Prin., Introd., 10. Prin., 22. Alc., VII., 6. Defence,
18-20.

Understanding and will are constituent functions of spirit : Prin., 27. Hylas, 457-458. Siris, 254.

Number is a creature of the mind : Vision, 109. Prin., 12, 118-120. Alc., VII., 5. Siris, 288, 356.

Arithmetic originated in signs needed for practical life : Prin., 121-122. Alc., VII., 5, 12.

The object of geometry is not abstract extension : Vision, 124-125, 150-160. Prin., 123-132. Anal. Queries, 3, 6, 7. Siris, 208.*

A finite space is not infinitely divisible—the contrary opinion is the source of geometrical paradoxes : Vision, 80-86 (asserting the *minimum visibile*). Prin., 123-132. Hylas, 478-479. Motu, 46. Alc., VII., 15. Anal. Queries, 5, 16-23.

There is no absolute space, time or motion (no pure space, time relative, etc.) : Prin., 97-99, 110-117. Hylas, 402-404. Motu, 52-65. Anal. Queries, 7, 8, 12-14. Siris, 109,* 249, 266, 270-271, 289, 292-293, 318.

Gravity is no argument for matter : Prin., 103-106. Hylas, 459-460. Motu, 4, 6, *et passim*. Alc., VII., 6 (of force). Siris, 231-250, 319.

Immaterialism offers an abridgement of the sciences : Prin., 102, 134. Hylas, Pref., 479-480. Defence, 9.

The imperfection of language makes circumlocution and seeming contradiction unavoidable, and those who want to understand his doctrine must 'candidly collect my meaning from the whole sum and tenor of my discourse' : Vision, 120. Prin., Pref., 52. Hylas, 462. Siris, 296.

NOTE A.

Ideas in the Mind of God, the Platonic idea and ideas of sense.

In Prin., 70-76 Berkeley denies 'intelligible matter', but does not deny other ideas in the mind of God ; however he is clearly unwilling to affirm them. In Hylas, 452, he asserts that the real tree is in the mind of God, and in 459 he equates 'God hath ideas' with 'God knows', and in 468 ff. he very tentatively, and as a concession, admits an 'external archetype' of creation. All this must be interpreted by his words to Johnson (1730), 'I have no objection against calling the ideas in the mind of God archetypes of ours. But I object against those archetypes by philosophers supposed to be real things, and to have an absolute rational existence, distinct from their being perceived by any mind whatsoever'.

He speaks with similar reserve in *Siris*. He nowhere accepts the Platonic ideas. In *Siris*, 337, he speaks of the Divine ideas, as we might speak of ideals or values, and he himself refers to 313 and 330. 313 deals with the Peripatetic entelechies 'in the mind' and the Platonic 'innate notions'; 330 is a perfectly general passage in praise of contemplation of the supersensible world. He wants Plato's 'uplift' without Plato's ideas. In 338 he speaks sympathetically of them; for he wants to lessen 'the prejudice that now lies against' Plato, and to commend 'those exalted notions and fine hints that sparkle and shine throughout his writings'. If Plato's ideas were independent of God, Berkeley would condemn them as 'unknown'; if they were agents, he would condemn them as usurping the divine prerogative. The latter point, as perhaps we may infer from 335, gives the true reason for his non-acceptance of the Platonic ideas; in this passage he contrasts Plato's usage with that of his own philosophy, and says 'In Plato's style, the term idea doth not merely signify an inert inactive object of the understanding, but is used as synonymous with aition and arche, cause and principle'. That, I expect, is why, when in 266 he lists what he approves in Platonism, he does not include ideas. (I hardly think that he intends 'forms' to be the equivalent of 'ideas'.)

But what of his own ideas of sense? Has he not abandoned them in the *Siris*? No. A rose by any other name will smell as sweet. His doctrine of the sensible world is unchanged; his terminology is slightly altered. Once in *Siris*, and once only, I think, namely, in 308, he uses his old phrase 'ideas or passive objects'; but he several times calls the objects of sense 'appearances in the mind', and I should call 251 (see his own table of contents) his formal statement of immaterialism and the *esse percipi* in terms of appearances in the mind. He is never tired of emphasizing the passivity of the objects of sense; it is one of the main themes of the *Siris*; and etymologically there is not much difference between the Greek word 'idea,' and the anglicized Latin 'appearance'. The term idea was never essential to the philosophy. Berkeley often apologized for it; he knew he was 'using ways of speech not common'. But, as he says, every one called immediate objects 'ideas' in those days, just as we all call them sense-data to-day; and since Berkeley very reasonably held that things were composed of sense-data, it was perfectly right for him to say among the learned that 'we eat and drink and are clothed with ideas'. But as the years went by, he found out that the term idea was more of a liability than an asset. It was a weapon good in attack, but poor in defence;

it was the Jonah of his ship ; he realized that people never would consent to call the objects of sense ideas, and that he who tried to make them do so was running his head against a wall. But he had and always had several alternative terms ; and without any fuss he gradually unburdened himself of the liability and jettisoned the Jonah. We only find the technical 'things or ideas' a very odd time in the *Alciphron*, and not at all in the *De Motu* ; and so we may conclude that the *Siris* is only completing a terminological improvement, begun long before. 'Appearances perceived by sense, are they not ideas ?' asks Philonous (Hylas, 460). We may therefore adapt Berkeley's oft-quoted saying about ideas and things, and say that in the *Siris* his aim is not to change things into appearances, but rather appearances into things, seen in God.

NOTE B.

Pure intellect, notions, and the cognitive faculties.

Pure intellect appears twice in the *Commonplace Book*, by implication accepted in 536, by implication rejected in 822. In Hylas, 404, it is with hesitation and qualification accepted. It is accepted and used vaguely in *De Motu*, 53, and the term occurs in Alc., VII., 5 (1st ed.), in the mouth of Alciphron for the faculty of abstract ideas. In all this Berkeley was consistent and wise ; he wished to affirm our powers of spiritual apprehension without leaving a loophole for false abstraction. Berkeley never aimed at technical precision in his use of terms descriptive of our cognitive faculties ; the imagination is, in his view, sometimes a help to thought, sometimes a hindrance. Sometimes he uses perception as equivalent to sensation, sometimes for a blend of sense and image ; he rarely stresses memory. Mind is used vaguely for 'all the ways', including sense. Sometimes, as in *Siris*, 303, he admits a supra-discursive faculty. 'What we see, we know' (Vindic. 20) is only verbally opposed to *Siris*, 305, 'Sense knoweth not' ; for he has just said, *Siris*, 264, 'Sense and experience acquaint us . . .'. Throughout, the fixed points of his concrete thinking were sense, imagination and reason, combining to give knowledge by fit use of signs. His epistemology is most consistently worked out in Alc., VII., 11-14, and a résumé of that passage appears in *Siris*, 303. He cannot be tied down in any period of his life (see Prin., 25 ; Hylas, 448 ; Alc., VII., 5 ; *Siris*, 290), or any edition of his books to a precise use of the term 'notion'. The word must take

its colour from its context. He tried in the second edition of the *Principles*, as he had done in the *Hylas*, to give it some precision, opposed to knowledge of ideas; but the notion was never a *tertium quid* of knowledge to him, nor a real thing, like the idea. Dr. Hedenius in his *Sensationalism and Theology in Berkeley's Philosophy*, page 120 n., expresses partial agreement with what I have elsewhere written on notions.

NOTE C.

Relations.

The addition of 'relations' in Prin., 89, 2nd ed. (1734), at first sight looks like a substantial change of doctrine; but I think it is only an instance of that 'touching up', which Berkeley liked doing. We may trace the addition to the close study of epistemology, represented by Alc., VII., 11-15 (1732). That study had fixed his attention on the fact, always known to him, that there is no 'pure sensation' any more than there is 'pure intellect' in the real life of mind, and that if we want to *know*, 'mere consideration of the original ideas' will not suffice. He does not specify relations, I think, in the 1st and 2nd editions of the *Alciphron*, but in the 3rd edition (1752) he inserts brief mentions of them in VII., 12 and 14. The former insertion rather neutralizes the insertion in the *Principles*. In 1734 he writes that relations are 'distinct from the ideas or things related'; in 1752 he writes that relations are 'not abstract general ideas, being founded in particular things, and not making of themselves distinct ideas to the mind exclusive of the particular ideas and the signs'. If Cloyne is an idea and Cork an idea, then the road joining Cloyne and Cork is an idea, and Berkeley never doubted it. The relation road is 'founded in particular things' because it runs into both towns; but it is not a third nature, and I doubt if Berkeley even toyed with that notion. I conclude that Berkeley's introduction of 'relations' alongside of spirit and ideas was half-hearted, not sustained, and meant as a refinement, not an alteration.

NOTE D.

This paper was written before I read Prof. Stocks' subtle discussion of the *esse percipi* in MIND (July, 1936). His exposure of the fallacy in Russell's rejoinder is correct and timely. Russell suggests that Berkeley confused the idea with the act

of mind. Stocks points out that the fact is otherwise. What puzzles me is this: Stocks knows so well that the contrast between act of mind and passive idea is fundamental in Berkeley's publications; how then can he maintain that Berkeley in his deliberate, meditated, opening section of the *Principles* means to describe the operations of the mind as ideas? Stocks begins his quotations from the *Commonplace Book* at 584; had he gone back to 492 he would have read: 'Qu. Whether it were not better *not* to call the operations of the mind ideas, confining this term to things sensible?' This point is connected with Berkeley's doctrine of self-knowledge, on which subject Stocks has laid himself open to misunderstanding. He says that Berkeley gave up 'internal sense (reflection)' as the means of self-knowledge. I grant that Berkeley did not like the term sense in that context (Prin., 136); but he always maintained self-knowledge by inner reflection, interpreted as immediate feeling (*Conscientia quadam interna*, De Motu, 21; 'inward feeling or reflection,' Prin., 89, 2nd ed.; 'reflection' and 'reflex act,' Hylas, all eds.). I think my paper supplies a corrective to the references to Berkeley's 'vacillation and inconsistency', and my table of passages will show that Berkeley makes far more use in the *Principles* of his doctrine of abstract ideas than Stocks seems disposed to allow.

IV.—TAUTOLOGIES AND THE MATRIX METHOD.

BY MORRIS LAZEROWITZ.

IN his *Tractatus* Mr. Wittgenstein constructs a logical method for defining all possible truth-functions of propositions. This, namely the so-called Matrix Method, supplies a formal procedure for investigating the truth-status of any complex, p , of propositions connected by such logical connectives as occur in the first five numbers of *Principia Mathematica*, for each of p 's possible truth-conditions. That is, it is a formal method for determining what would be the truth-value of a proposition p , complex, or, in the limiting case, simple, if a given possible truth-condition ϕ of p were realised—this for all ϕ 's of p . Definitions of truth-functions obtained by application of this method are in the form of truth-tables which analytically exhibit the logical conditions under which a given truth-function $f(p, q, \dots)$ would be true, together with those under which it would be false. In this manner the logical meaning of the truth-function is analytically given, *e.g.* :—

p	q	r	$p \cdot q \supset r$
T	T	T	T
T	F	F	T
T	T	F	F
T	F	T	T
F	F	F	T
F	T	T	T
F	F	T	T
F	T	F	T

It should be observed that this method is applicable only to those propositional complexes which are definable by reference to truth-conditions, *i.e.*, to propositions whose truth-values are logically *determined* by the truth-values of their arguments. For this method concerns the explication of the possible truth-value relations, *i.e.*, relations of truth-value determination,

existing between a truth-function $f(p, q, \dots)$ and its arguments p, q, \dots , the possible truth-values of which when combined in all possible ways constitute the set of truth-conditions by reference to which $f(p, q, \dots)$ is defined. Consequently only those propositions p are definable by the matrix method which are values of

$$(\phi) : \phi(p) \cdot \supset (\exists t) \cdot \phi \supset t(p).^1$$

Among the functions amenable to matrix analysis Mr. Wittgenstein includes tautologous and self-contradictory functions.² That he should do so seems indeed quite warranted, since tautologies and contradictions appear to involve no logical constants other than those explicitly defined in *Principia*. However, one consequence entailed by this inclusion is that tautologous and contradictory functions must be counted as *truth-functions*³ and be considered as having truth-conditions by reference to which they are definable. This appears to me highly questionable. It seems to me that tautologous and self-contradictory functions are not truth-functions. And it therefore seems to me that the matrix method, if applicable to them, is not applicable to them in the sense in which it is applicable to truth-functions, namely in the sense in which truth-functions are defined by reference to their truth-conditions. It will be my purpose in the following paragraphs to demonstrate this, and also to show in what form the matrix method can be employed to define tautologies and contradictions.

It is usual, of course, to consider tautologies as *true* propositions and contradictions as *false* ones, i.e., to classify them respectively with true and false propositions. This accords with Mr. Wittgenstein's matrix definitions of tautologies and contradictions, namely: a tautology is a proposition whose truth-value is truth for all its truth-conditions; a contradiction is a proposition whose truth-value is falsehood for all its truth-conditions. If this view were correct, tautologous and self-contradictory functions would have to be construed as *truth-functions*, susceptible of matrix analysis, i.e., definable in terms of truth-value relations existing between them and their (sup-

¹ $\phi(p) = \phi$ is a truth-condition of p , t = a truth-value variable whose values are T and F.

² L. Wittgenstein, *Tractatus Logico-Philosophicus*, 5.101.

³ "We may call a function $f(p)$ a 'truth-function' when its argument p is a proposition, and the truth-value of $f(p)$ depends only upon the truth-value of p ." Russell and Whitehead, *Principia Mathematica* (Second edition), p. 8.

posed) truth-conditions. The senses of the words "true" and "false" in which tautologies are to be supposed true and contradictions false, have, however, never been made clear. And it may be the case, even if in *any* senses they *are* true and false respectively, that they are not true and false respectively in senses which would make them amenable to matrix analysis. It will therefore be important to investigate those senses, if there are any, in which tautologies can significantly be said to be true and contradictions false.

In a statement which, I take it, expresses the general view of those philosophers who hold the above view concerning tautologies and contradictions Mr. Ramsey asserts: "The assimilation of tautologies and contradictions with true and false propositions respectively results from the fact that tautologies and contradictions can be taken as arguments to truth-functions *just like ordinary propositions*, and for determining the truth or falsity of the truth-function, tautologies and contradictions among its arguments must be counted as true and false respectively."¹ Neither tautologies nor contradictions, however, are held to be *ordinary* propositions. For, in what I take to be a statement about *ordinary* propositions, Mr. Wittgenstein writes: "The proposition is a picture of reality. The proposition is a model of the reality as we think it is."² Accordingly, since it is quite clear that "Tautologies and contradictions are not pictures of the reality"³ because "They present no possible states of affairs,"³ it is held that "Tautologies and contradictions are *not real propositions*, but degenerate cases."⁴ By *real* propositions, namely propositions descriptive of reality, is obviously meant *contingent* propositions, *i.e.*, propositions the truth-values of which can only be ascertained by reference to facts *extrinsic* to the propositions, so that an inspection limited exclusively to the propositions will not disclose their truth-values.⁵ It seems plain, then, that holding tautologies and contradictions to be true and false respectively *just like ordinary propositions* is equivalent to holding them to be true and false respectively in the same senses in which *contingent* propositions are true or false. Is this a possible view?

It cannot, indeed, be held that tautologies *are* contingent truths, capable of being false, or that contradictions *are* contingent falsehoods, capable of being true. And Mr. Wittgenstein

¹ F. P. Ramsey, *The Foundations of Mathematics*, p. 11; italics my own.

² L. Wittgenstein, *op. cit.*, 4.01.

³ *Ibid.*, 4.462.

⁴ F. P. Ramsey, *op. cit.*, pp. 9-10; italics my own.

⁵ L. Wittgenstein, *op. cit.*, 2.222, 2.224.

explicitly distinguishes between tautologies, contradictions, and contingent propositions in this respect: "The truth of tautology is certain, of propositions possible, of contradiction impossible."¹ From this it seems Mr. Wittgenstein intends to hold that tautologies and contingent truths are not logically distinguishable with respect to the property "true," but are distinguishable with respect to some *other* properties, namely "certainty," or "necessity," and "possibility;" and also contradictions and contingent falsehoods are not distinguishable with respect to the property "false," but are distinguishable with respect to some *other* properties, namely "necessity," *i.e.*, "necessarily false," and "possibility," *i.e.*, "possibly false." These distinctions are in accordance with and explicitly exhibited in Mr. Wittgenstein's matrix definitions of tautologies, contradictions, and contingent propositions. Thus, for example, the following three matrices do not apparently differ from each other with respect to the meanings of the T's and F's but do differ with respect to the other properties:

(1)	<table> <tr><th>p</th><th>$p \vee \sim p$</th></tr> <tr><td>T</td><td>T</td></tr> <tr><td>F</td><td>T</td></tr> </table>	p	$p \vee \sim p$	T	T	F	T	(2)	<table> <tr><th>p</th><th>q</th><th>$p \vee q$</th></tr> <tr><td>T</td><td>T</td><td>T</td></tr> <tr><td>F</td><td>T</td><td>T</td></tr> <tr><td>T</td><td>F</td><td>T</td></tr> <tr><td>F</td><td>F</td><td>F</td></tr> </table>	p	q	$p \vee q$	T	T	T	F	T	T	T	F	T	F	F	F	(3)	<table> <tr><th>p</th><th>$p \cdot \sim p$</th></tr> <tr><td>T</td><td>F</td></tr> <tr><td>F</td><td>F</td></tr> </table>	p	$p \cdot \sim p$	T	F	F	F
p	$p \vee \sim p$																															
T	T																															
F	T																															
p	q	$p \vee q$																														
T	T	T																														
F	T	T																														
T	F	T																														
F	F	F																														
p	$p \cdot \sim p$																															
T	F																															
F	F																															

The first matrix (1) shows that no F could possibly occur in the right-hand column and therefore shows that $p \vee \sim p$ is true with "certainty" or is necessarily true. In (2) it is to be seen that both T's and F's occur in the right-hand column and thus that $p \vee q$ could *possibly* be true and also could *possibly* be false. The last matrix (3) shows that F's *only*, or that no T's, can occur in the right-hand column, and hence that $p \cdot \sim p$ is necessarily false, or impossible, *i.e.*, could not possibly be true. These distinctions between the logical ways in which propositions are supposed to be capable of being true or of being false concern the modal properties "necessity," "possibility," and "impossibility," which are formalised in Prof. Lewis's System of Strict Implication and there symbolised respectively " $\sim \diamond \sim$," " \diamond ," " $\sim \diamond$."

By adding these notations to the above matrices the modal distinctions between the ways in which tautologies, contingent propositions, and contradictions are held by Mr. Wittgenstein

¹ L. Wittgenstein, *op. cit.*, 4.464.

to be true or to be false can be more explicitly exhibited. Matrix (1) could be written in the form

p	$p \vee \sim p$
T	T
F	T

$$\sim \diamond \sim T, = \sim \diamond F,$$

which clearly shows that $p \vee \sim p$ is necessarily true or that it could not possibly be false; (2) could be written

p	q	$p \vee q$
T	T	T
F	T	T
T	F	T
F	F	F

$$\diamond T, \diamond F,$$

which shows that the truth or falsity of $p \vee q$ is contingent or possible only. Finally (3) could be written

p	$p \cdot \sim p$
T	F
F	F

$$\sim \diamond \sim F, = \sim \diamond T,$$

which explicitly exhibits that $p \cdot \sim p$ is necessarily false, or could not possibly be true.

One fact becomes immediately clear from an inspection of these matrices. Whereas in the case of contingent propositions the property "true" has an associated negative property "false," so that a contingent proposition could possibly be false though it is in point of fact true (and conversely), in the case of tautologies and contradictions the properties "true" and "false" have no associated negatives. For tautologies cannot possibly be false, *i.e.*, "false" is not significantly predicable of tautologies, and contradictions cannot possibly be true, *i.e.*, "true" is not significantly predicable of contradictions. This distinction between the ways in which propositions are supposed to be capable of being true or false is equivalent to the following frequently made distinction: the truth-value of a contingent proposition is not intrinsically certifiable¹; the truth-values of tautologies and contradictions are intrinsically certifiable.²

The meanings of "true" and "false" as used in connection with ordinary propositions are clear, although by no means analytically so. It may not, of course, be the case that there is a single property "true" which is the property of *all* contingent propositions that are true, or a *single* property "false" which

¹ L. Wittgenstein, *op. cit.*, 2-224.

² *Ibid.*, 3-04, 3-05.

is the property of *all* contingent propositions that are false. It may be the case that there is a class α of several properties each of which is designated by the word "true" and each of which is applicable to certain kinds of contingent propositions and not to other kinds; similarly for a class β of "false" properties. And in view of this possibility I want to ask the following question: Is there a property "true" which is the property of tautologies and which is also a member of α ? And I also want to ask whether there is a property "false" which is the property of contradictions and is also a member of β .

Clearly any contingent proposition p , if true, is true because it stands in *some* relation of unique correspondence, in any of a number of senses of "correspondence," to a fact or set of facts extrinsic to p and existing in the so-called real world; and, if false, is false because it fails to stand in such a relation. Concerning this Mr. Wittgenstein states: "In the agreement or disagreement of its sense with reality, its truth or falsity consists."¹ Thus any property "true" or "false" that is applicable to a contingent proposition p will be had by p in respect of p 's standing in some relation of agreement or disagreement with the real world, in some or other senses of "agreement" and "disagreement" in which p , if true, is *rendered* true by some state of affairs in the real world, and if false is *rendered* false by some state of affairs in the real world. In consequence, any property that is a member either of α or of β will be either identical with or equivalent to some *relational* property, of the kind agreement, or some *relational* property, of the kind disagreement, with the real world. Plainly, however, the property "true" supposedly attaching to tautologies, as well as the property "false" supposedly attaching to contradictions, cannot be either identical with or equivalent to some relational property of the kind agreement, or disagreement, with reality. For tautologies, if in any sense true, are *intrinsically* true, and contradictions, if in any sense false, are *intrinsically* false. They therefore are not true and false respectively in respect of standing in some relations to the real world which is *extrinsic* to them. This is to say, neither the truth of a tautology nor the falsity of a contradiction is *determined* by anything extrinsic to it; states of affairs in reality do not *render*, or *make*, a tautology true or a contradiction false. Hence the truth of a tautology does *not* "consist" in its agreement with the real world, or with anything extrinsic to it, nor does the falsity of a contradiction "consist" in its disagreement with the real world, or with anything ex-

¹ L. Wittgenstein, *op. cit.*, 2.222.

trinsic to it. Mr. Wittgenstein apparently recognises this fact in the following statement: "In the tautology the conditions of agreement with the real world—the presenting relations—*cancel* one another, so that it stands in no presenting relations to reality."¹ An inspection of the matrix definition of any tautology, in which it will be seen that the (supposed) truth-value of the tautology remains *unchanged* for *all* its truth-conditions, will corroborate his statement. This consideration of course also holds for the property "false" which contradictions are thought to have. Consequently, since neither the property "true" attaching to tautologies nor the property "false" attaching to contradictions is equivalent to or identical with a relational property of agreement or of disagreement with reality, neither property can be a member respectively of α or of β . And obviously, therefore, tautologies and contradictions are not true and false respectively in *any* sense in which contingent propositions are true or false.

It now becomes difficult to see in *what* senses "true" and "false" are used when they are used in connection with tautologies and contradictions respectively, and also just what is meant by "necessarily, or intrinsically, true" and "necessarily, or intrinsically, false." At any rate it is plain that tautologous and self-contradictory functions are not *truth*-functions, in any usual sense; because any proposition resulting from the substitution of values in either a tautologous or self-contradictory function will not give rise to a true or false proposition, in any usual sense of "true" and "false." And even if it be granted that the senses of "true" and "false," as used in connection with tautologies and contradictions, are perfectly definite and clear, it nevertheless must be admitted that the truth-values of tautologies and contradictions are not logically *determined* by truth-conditions. That is to say, since tautologies, if true, are *intrinsically* true, their truth-status will not be affected by states of affairs *extrinsic* to them. They will thus not be determinable by reference to truth-conditions which themselves are such that their truth-values *are* determined by states of affairs extrinsic to them. Tautologous functions will not therefore be truth-functions, definable by reference to truth-conditions. This consideration holds also, of course, for contradictory functions.

Mr. Wittgenstein seems to recognise this fact, as I think the following statement shows: "The tautology *has no truth-conditions*, for it is unconditionally true; and the contradiction is

¹ L. Wittgenstein, *op. cit.*, 4·462; italics my own.

on no condition true."¹ This seems to me tantamount precisely to asserting that the truth-values of tautologies and contradictions, whatever senses "truth-value" may have in such connections, are not determined by the truth-values of the propositions upon which they are constructed, and so are not truth-functions of them. And plainly a truth-conditionless function cannot be taken as a truth-function.

If the above considerations are correct, it follows that the matrix method is not genuinely applicable to tautologies and contradictions, since it is applicable to those propositions only which are truth-functions of their arguments, *i.e.*, to those propositions only which when substituted for p in

$$(a) \quad (\phi) : \phi(p) \cdot \supset \cdot (\exists t) \cdot \phi \supset t(p)$$

give rise to true propositions. Of course, both tautologies and contradictions are significantly substitutable for p in (a), and moreover the results of such substitutions will be *true* propositions. Thus it might be held that though tautologies and contradictions are not, *strictly* speaking, truth-functions they can nevertheless be treated logically as truth-functions, and so be given matrix definitions. For it is *true* to say that the truth-value of a tautology, as well as that of a contradiction, is determined by *all* its truth-conditions, or that a tautology is true and a contradiction false for *all* its truth-conditions, *e.g.*

$$(a_1) \quad (\phi) : \phi(q \cdot \supset \cdot p \vee q) \cdot \supset \cdot (\exists t) \cdot \phi \supset t(q \cdot \supset \cdot p \vee q),$$

$$(a_2) \quad (\phi) : \phi[p \vee q : \sim (q \vee p)] \cdot \supset \cdot (\exists t) \cdot \phi \supset t[p \vee q : \sim (q \vee p)].$$

Consider, however, the reason *why* (a₁) and (a₂) are true propositions. (a₁) and (a₂) are equivalent respectively to

$$(a_{1s}) \quad \sim (\exists \phi) : \phi(q \cdot \supset \cdot p \vee q) : \sim (\exists t) \cdot \phi \supset t(q \cdot \supset \cdot p \vee q),$$

$$(a_{2s}) \quad \sim (\exists \phi) : \phi(p \vee q : \sim p \cdot \sim q) : \sim (\exists t) \cdot \phi \supset t(p \vee q : \sim p \cdot \sim q);$$

and these are true propositions because it is true that $q \cdot \supset \cdot p \vee q$ and $p \vee q : \sim p \cdot \sim q$ have no truth-conditions, *i.e.*, because it is true that

$$(c) \quad \sim (\exists \phi) \cdot \phi(q \cdot \supset \cdot p \vee q)$$

and

$$(c_1) \quad \sim (\exists \phi) \cdot \phi(p \vee q : \sim p \cdot \sim q).$$

But if (a_{1s}) and (a_{2s}) are true *because* (c) and (c₁) are true then the following propositions are also true:

$$(a^1) \quad (\phi) : \phi(q \cdot \supset \cdot p \vee q) \cdot \supset \cdot \sim (\exists t) \cdot \phi \supset t(q \cdot \supset \cdot p \vee q),$$

$$(a^2) \quad (\phi) : \phi(p \vee q : \sim p \cdot \sim q) \cdot \supset \cdot \sim (\exists t) \cdot \phi \supset t(p \vee q : \sim p \cdot \sim q).$$

These propositions, (a¹) and (a²) are rendered true by the same conditions that render (a_{1s}) and (a_{2s}) true, namely by

¹ L. Wittgenstein, *op. cit.*, 4.461; italics my own.

(c) and (c₁) respectively. Consequently, though (a₁) and (a₂) are true propositions, and though it is thus true to say that tautologies and contradictions are amenable to matrix analysis, it is *trivial* to say so. For (a) is satisfied only *vacuously* by tautologies and contradictions, with the consequence that tautologies and contradictions, are only *vacuously* definable by the matrix method.

Let us consider now a genuine matrix definition with a view to seeing precisely what its logical structure is, e.g. :

	p	q	$p \vee q$
(1)	T	T	T
(2)	F	T	T
(3)	T	F	T
(4)	F	F	F

The first horizontal line of truth-values (1) is actually the proposition

$$(1) \quad p \cdot q \supset p \vee q,$$

while the remaining lines are respectively the propositions

$$(2) \quad \sim p \cdot q \supset p \vee q,$$

$$(3) \quad p \cdot \sim q \supset p \vee q,$$

$$(4) \quad \sim p \cdot \sim q \supset \sim (p \vee q).$$

I.e.,

	p	q	$p \vee q$	
(1)	T	T	T	$= p \cdot q \supset p \vee q$
(2)	F	T	T	$= \sim p \cdot q \supset p \vee q$
(3)	T	F	T	$= p \cdot \sim q \supset p \vee q$
(4)	F	F	F	$= \sim p \cdot \sim q \supset \sim (p \vee q).$

Each of these propositions, (1)-(4), is a tautology, in which " \supset " must be interpreted as a necessary connection between antecedent and consequent, rather than as a material one. And the truth-conditions in this matrix, i.e., $p \cdot q$, $\sim p \cdot q$, $p \cdot \sim q$, $\sim p \cdot \sim q$, can easily be seen to be *genuine* and actually to be required in the matrix analysis of $p \vee q$. For each necessitates that $p \vee q$ have a certain truth-value, such that each condition taken with the negative of the truth-value it logically imposes on the truth-function, i.e., with the negative of its consequent, results in a contradiction between the two :

$$\begin{aligned} (1) \quad & p \cdot q : \sim (p \vee q), \\ (2) \quad & \sim p \cdot q : \sim (p \vee q), \\ (3) \quad & p \cdot \sim q : \sim (p \vee q), \\ (4) \quad & \sim p \cdot \sim q : p \vee q. \end{aligned}$$

In each case, it is to be noted, the contradiction *vanishes* if the condition is dropped.

Consider, however, a usual matrix definition of a tautology, *e.g.*,

	p	q	$p \vee q \supset q \vee p$
(1)	T	T	T
(2)	F	T	T
(3)	T	F	T
(4)	F	F	T

in which the horizontal lines are respectively the propositions

- (1) $p \cdot q \supset p \vee q \supset q \vee p$,
- (2) $\sim p \cdot q \supset p \vee q \supset q \vee p$,
- (3) $p \cdot \sim q \supset p \vee q \supset q \vee p$,
- (4) $\sim p \cdot \sim q \supset p \vee q \supset q \vee p$.

An inspection of this set of propositions, (1)-(4), will disclose that this matrix is different from the preceding one in the following important respect: namely, the supposed truth-conditions are logically *redundant*, since they actually impose no truth-values on the function. The negatives of (1)-(4) are respectively

- (1) $p \cdot q : p \vee q : \sim q \cdot \sim p$,
- (2) $\sim p \cdot q : p \vee q : \sim q \cdot \sim p$,
- (3) $p \cdot \sim q : p \vee q : \sim q \cdot \sim p$,
- (4) $\sim p \cdot \sim q : p \vee q : \sim q \cdot \sim p$,

where in each case the involved contradiction does not exist *between* the supposed truth-condition and

- (4¹) $p \vee q : \sim q \cdot \sim p$.

For in each case the condition can be dropped without thereby eliminating the contradiction. That is, in (4), for example, there are not *two* contradictions, *one* between

$$\sim p \cdot \sim q \text{ and } p \vee q : \sim q \cdot \sim p$$

and *another* between

$$p \vee q \text{ and } \sim q \cdot \sim p,$$

such that if $\sim p \cdot \sim q$ were dropped one contradiction would vanish while the other would remain. There is *only one* contradiction in (4¹), that between $p \vee q$ and $\sim q \cdot \sim p$, which of course is identically the same as that between $p \vee q$ and $\sim p \cdot \sim q$ —the only difference being a notational one; *i.e.*,

$$\begin{array}{l} p \vee q : \sim q \cdot \sim p \\ \text{and} \quad p \vee q : \sim p \cdot \sim q \end{array}$$

involve between them the *same* contradiction. Hence by adjoining $\sim p . \sim q$ to (4¹) no new logical relation obtains, and by dropping $\sim p . \sim q$ from (4), which leaves (4¹), no logical relation is caused to vanish from (4). It is thus plain that (4) and (4¹) are distinct notationally only, and therefore that $\sim p . \sim q$ is logically redundant. $\sim p . \sim q$ must for this reason be considered to be no truth-condition of $p \vee q . \supset . q \vee p$, because if $\sim p . \sim q$ were the case it would impose no truth-value on the implication existing between $p \vee q$ and $q \vee p$. Similar (not identical) considerations can easily be seen to hold for the remaining cases, and in general for the supposed truth-conditions of all tautologies and contradictions. The supposed truth-conditions of tautologies and contradictions are in no literal sense truth-conditions of them, such as logically to *condition* their truth-values—if it makes sense to ascribe truth-values to them. The supposed function of such truth-conditions in matrix definitions, in the sense discussed above, is a fiction. They are logically redundant, and matrices of tautologies and contradictions seem actually to be nothing more than statements like, *e.g.*, the following :

$$\frac{p \vee q . \supset . q \vee p}{T}$$

and

$$\frac{q : \sim p . \sim q}{F}$$

Such matrices, if they can be so called, are obviously logically useless so far as concerns matrix *analyses* of tautologies and contradictions.

It is clear then that matrix technique, in its ordinary sense, is not applicable to tautologies and contradictions. It can however be so extended as to be made analytically applicable to them, as I shall attempt to show in the remaining paragraphs of this paper.

The arguments of propositional functions of the kinds under discussion can be divided into two kinds, namely into what I shall term "primary" and "non-primary." A set of primary arguments of a function $f(p, q . . .)$ will consist of arguments which are not themselves constructs (of arguments) containing logical constants, *e.g.*, the arguments p, q of $p \equiv q$. A great many functions, besides having primary arguments, will also involve non-primary arguments, namely arguments which are themselves constructs (of arguments) involving logical constants, such as \sim, \vee, \supset , *e.g.*, the arguments $p \vee q$ and $r \supset s$ of $p \vee q . \supset . r \supset s$.

This is to say, non-primary arguments will be those containing logical constants, whereas primary arguments will contain no constants. Thus, $p \vee q \supset r \supset s$ is constructed on two sets of arguments: the primary set p, q, r, s , and the non-primary set $p \vee q, r \supset s$, which in this case is the n -th set of arguments of the function. Obviously the n -th arguments of a function $f(p, q \dots)$ will be those arguments, primary or non-primary, between which the logical connective having the widest scope in $f(p, q \dots)$ holds; and of course the n -th set may contain both primary and non-primary arguments, *e.g.*, the arguments $q, p \vee q$ of $q \supset p \vee q$.

With respect to any set of possible truth-value combinations of primary arguments no logical distinction can be made between truth-functions, tautologous functions, and contradictory functions constructed on those arguments. In each case the set will consist of 2^n possibilities (n = number of arguments). Thus, *e.g.*, the possible truth-value combinations of the primary arguments of $p \equiv q, q \supset p \vee q$, and $p \vee q : \sim q : \sim p$ are identical:

$$\left. \begin{array}{c|c} p & q \\ \hline T & T \\ F & T \\ T & F \\ F & F \end{array} \right\} = p \cdot q \cdot \vee \cdot \sim p \cdot q \cdot \vee \cdot p \cdot \sim q \cdot \vee \cdot \sim p \cdot \sim q \cdot$$

With respect to the totality of possible truth-value combinations of the arguments belonging to their n -th sets, however, truth-, tautologous, and contradictory functions can be logically distinguished, because such sets will *not be identical*. A function $f(p, q, \dots)$ will be a truth-function of p, q, \dots if and only if the arguments of its n -th set, say r in number, are capable of entering into 2^r possible truth-value combinations with each other, *i.e.*, if all the truth-value combinations of the arguments belonging to its n -th set are *possible* truth-value combinations. For then the arguments place no logical restrictions on each other, and the connection between them is *material*, capable of being false, or of failing to hold. The arguments of the n -th set of arguments upon which tautologies and contradictions are constructed obviously do place logical restrictions on each other; because if there were no logical restrictions of some sort between them the logical relations holding between them would be material, capable of not holding, or of being false. Thus no proposition will be a contradiction or a tautology unless some truth-value combinations of the arguments belonging to its n -th set are not logically possible combinations, *i.e.*, if for the arguments, r in number, there are less than 2^r possible truth-value combinations.

Combining the distinction between primary and non-primary sets of arguments of functions with the matrix method, let us see how tautologies and contradictions can be analysed (without assigning fictitious truth-values to them), *e.g.*,

$$(1) \quad q \supset r . \supset : p \vee q . \supset . p \vee r$$

and

$$(2) \quad p \vee q : \sim q . \sim p .$$

The first proposition, (1), is an implication constructed on the non-primary arguments $q \supset r$ and $p \vee q . \supset . p \vee r$, the second of which is itself an implication constructed on the non-primary arguments $p \vee q$ and $p \vee r$. Each of these arguments, *i.e.*, $q \supset r$, $p \vee q$, and $p \vee r$, can readily be seen to be truth-functions, because their respective n -th arguments combine in 2^n possible truth-value combinations. They therefore can be given ordinary matrix, or truth-value, definitions:

q	r	$q \supset r$	p	q	$p \vee q$	p	r	$p \vee r$
T	T	T	T	T	T	T	T	T
F	T	T	F	T	T	F	T	T
T	F	F	T	F	T	T	F	T
F	F	T	F	F	F	F	F	F

These non-primary arguments have arguments in common, and since the set of primary arguments p, q, r combine in 2^3 possible truth-value combinations we can construct a single matrix defining $q \supset r$, $p \vee q$, and $p \vee r$:

p	q	r	$q \supset r$	$p \vee q$	$p \vee r$
T	T	T	T	T	T
T	F	F	T	T	T
F	T	T	T	T	T
T	F	T	T	T	T
T	T	F	F	T	T
F	F	F	T	F	F
F	F	T	T	F	T
F	T	F	F	T	F

We can now ascertain whether or not the n -th argument $p \vee q . \supset . p \vee r$ of $q \supset r . \supset : p \vee q . \supset . p \vee r$ is a truth-function, amenable to truth-value matrix definition. We see that it is a truth-function, because with respect to the totality of truth-value combinations of p, q, r , none of which are impossible, the n -th arguments $p \vee q$ and $p \vee r$ of $p \vee q . \supset . p \vee r$ can enter into all truth-value combinations, none of which are impossible; *i.e.*, $p \vee q$ and $p \vee r$ are logically independent so far as concerns their respective possible truth-values:

	$p \vee q$	$p \vee r$	$p \vee q \supset p \vee r$
	T	T	T
(B)	F	F	T
	F	T	T
	T	F	F

i.e.,

	p	q	r	$p \vee q$	$p \vee r$	$p \vee q \supset p \vee r$
	T	T	T	T	T	
	T	F	F	T	T	
	F	T	T	T	T	T
(C)	T	F	T	T	T	
	T	T	F	T	T	
	F	F	F	F	F	T
	F	F	T	F	T	T
	F	T	F	T	F	F

A comparison of matrices (A) and (C), which, combined into a single matrix, is:

	p	q	r	$q \supset r$	$p \vee q$	$p \vee r$	$p \vee q \supset p \vee r$
	T	T	T	T	T	T	
	T	F	F	T	T	T	
	F	T	T	T	T	T	T
(D)	T	F	T	T	T	T	
	T	T	F	F	T	T	T
	F	F	F	T	F	F	T
	F	F	T	T	F	T	T
	F	T	F	F	T	F	F

will show that with respect to the totality of truth-value combinations of p, q, r the n -th arguments of $q \supset r, \supset: p \vee q \supset p \vee r$, namely $q \supset r$ and $p \vee q \supset p \vee r$, cannot enter into 2^2 possible truth-value combinations. That is, for the two n -th arguments, each of which has two possible truth-values, there are less than 2^2 possible truth-value combinations:

	p	q	r	$q \supset r$	$p \vee q \supset p \vee r$
	T	T	T		
	T	F	F		
	T	F	T		
	F	T	T	T	T
(E)	T	T	F	F	T
	F	F	F	T	T
	F	F	T	T	T
	F	T	F	F	F

where it can be seen that $q \supset r$ cannot be true jointly with the falsity of $p \vee q \supset q \vee p$. The n -th arguments upon which $q \supset r \supset p \vee q \supset p \vee r$ is constructed place logical restrictions upon each other; it therefore is not a truth-function, and not capable of truth-value matrix definition. Moreover, (E) shows that the relation of implication holding between the n -th arguments is a *necessary* connection, and therefore that $q \supset r \supset p \vee q \supset p \vee r$ is a tautology:

$$\sim \diamond \sim (q \supset r \supset p \vee q \supset p \vee r).$$

This somewhat complicated procedure can of course be simplified by telescoping the various matrices (A)-(E) into a single matrix:

p	q	r	$q \supset r$	\supset	$p \vee q$	\supset	$p \vee r$
T	T	T	T	$\sim \diamond \sim$	T	T	T
T	F	F	T		T		T
T	F	T	T		T		T
F	T	T	T		T		T
T	T	F	F		T	T	T
F	F	F	T		F	T	F
F	F	T	T		F	T	T
F	T	F	F		T	F	F

In the same manner we can apply matrix technique to the contradiction (2) $p \vee q : \sim q \cdot \sim p$:

p	q	$p \vee q$	$:$	$\sim q \cdot \sim p$
T	T	T	$\sim \diamond$	F
F	T	T		F
T	F	T		F
F	F	F		T

This matrix shows that the n -th arguments of $p \vee q : \sim q \cdot \sim p$, namely $p \vee q$ and $\sim q \cdot \sim p$, cannot enter into 2^2 possible truth-value combinations, and therefore that $p \vee q : \sim p \cdot \sim q$ is not a truth-function. It also shows that the n -th arguments $p \vee q$ and $\sim q \cdot \sim p$ cannot possibly be conjunctively combined, since they can neither be jointly true nor jointly false; and consequently that $p \vee q : \sim q \cdot \sim p$ is a self-contradiction, in which the main conjunction could not possibly hold:

$$\sim \diamond (p \vee q : \sim q \cdot \sim p).$$

V.—DISCUSSIONS.

NOTE ON THE ALLEGED CARTESIAN CIRCLE.

THE most obvious criticism of Descartes's metaphysical argument is that the proof of God's existence involves a logical circle; the point was taken up as soon as the *Meditations* appeared, and the charge answered by Descartes himself. Unfortunately, however, this 'official' answer has not proved altogether easy to understand. It is still giving trouble to the exponents of Cartesianism, and some modern writers have in their distress resorted to an interpretation which I do not think can possibly be derived from Descartes's own words. The fact that this interpretation now shows signs of becoming orthodox is my excuse for serving up once more some very familiar material.

Up to a point Descartes's meaning is not in dispute. He begins with a denial: he denies that he ever said¹ that *all* clear and distinct ideas require the guarantee of God's veracity and must be regarded as doubtful until they have received it. If this were so, there would indeed be no evidence which he could legitimately use to prove the existence of God. He must have *some* knowledge which is guaranteed by its clearness and distinctness independently of God's veracity; moreover, this independent knowledge must include not merely the unique intuition of his own existence, but also, at least, those causal axioms which he actually uses for the proof in the *Third Meditation*. The question then arises: If we can in these cases rely upon clearness and distinctness alone as a guarantee of truth, why not always? What precisely is it which requires the further guarantee of God's veracity? Here Descartes draws a distinction upon which he rests his whole defence against the charge of circularity. We must distinguish, he says, between those truths which we are actually at the moment perceiving clearly and distinctly and those which we merely *remember* we have so perceived in the past. With regard to the former, the understanding is self-sufficient and infallible; an atheist may assure himself of the truth of the mathematical axioms and of the validity of a very

¹ His expressions on the subject in the *Discourse* were certainly very indiscreet; in the *Meditations* and *Principles* he is more careful, though I think a reader of the *Meditations* might be excused if he did not take the point at once. In the *Replies to Objections* Descartes's assertions are clear and emphatic.

short and simple argument. It is clear, however, that in any considerable piece of reasoning we are bound to use some premises the full evidence for which is not before our minds at the moment when we draw the conclusion; and we must then rely upon our memory that we have proved them in the past. It is in such cases that the appeal to faith becomes necessary. It is not necessary in order to prove the existence of God; for where the whole of the evidence is capable of being gathered up into a single moment of insight, it is its own guarantee; and we can in this way prove the existence of God without *petitio*.¹

Such, in outline, is Descartes's answer to the charge of circularity in all the passages in which he deals with it; it is in the further interpretation of these statements that difficulty arises. There are two points which, it seems, Descartes has not made unambiguously clear: (1) what precisely it is which makes the remembered intuitions doubtful, and (2) how precisely this element of doubt is supposed to be removed by a belief in the existence and veracity of God. And since, as it appears, our understanding has been asserted to be infallible in all its actual intuitions of the clear and distinct, the stress which Descartes lays upon the difference between present and remembered intuitions has led some modern writers to conclude that the function of God's veracity is to guarantee not the human understanding but the human *memory*. Thus, if I ever had in the past a clear and distinct intuition of the truth of a proposition the evidence for which I do not now see, that proposition is indubitably certain; the question is 'Did I ever have this past intuition or is my memory deceiving me?' Sometimes, as I well know, my memory does deceive me; sometimes I think I remember events in my past history which did not happen at all, or events which happened but were not quite as I think I remember them to have been. This is the doubt which must infect all reasoning in which I depend on previously demonstrated conclusions, and it is for my memory, and my memory alone, that I need God's guarantee.²

¹ For a clear statement of this position, see, for example, *Entretien avec Burman* (A.T.V., p. 178):

'Si enim ignoraremus veritatem omnem oriri a Deo, quamvis tam clarae essent ideae nostrae, non sciremus eas esse veras, nec nos non falli, scilicet cum ad eas non adverteremus, et quando solum recordaremur nos illas clare et distincte percepisse. Alias enim, etiamsi nesciamus esse Deum, quando ad ipsas veritates advertimus, non possumus de iis dubitare; nam alias non possemus demonstrare Deum esse.'

² Thus Mr. S. V. Keeling in his recent valuable study of Descartes: 'It is our memory alone that divine veracity guarantees; intuition neither needs nor is susceptible of support'. (*Descartes*, p. 247).

This interpretation claims the great authority of M. Gilson. M. Gilson does not appear to be satisfied that all Descartes's statements are consistent with it; but nevertheless maintains that it represents the only answer to the charge of circularity which Descartes ever gave. See his *Commentary on the Discourse*, pp. 360-362, and cf. pp. 483-484.

I contest this view on the ground that it is excluded by any natural and reasonable interpretation of Descartes's own statements. It may (or may not) represent the only valid answer to the criticism in question, but I am sure it is not the answer which Descartes actually gave, and ought never to be put forward as such.

It is no part of my case to argue that Descartes *could not* have taken the line suggested. I note the point because it might at first sight appear incredible that God should be supposed to guarantee, of all things, the human memory; the unreliability of our memories is surely a patent fact, surviving all the consolations of the Christian faith. The objection is obvious, and has been pointed out by some who support this interpretation;¹ yet I think it rests on a misunderstanding. We must bear in mind that God's guarantee of a human faculty is never supposed by Descartes to be such as to exclude all possibility of error or to render unnecessary any precautions in the use of it. Sense-perception itself is in the *Sixth Meditation* brought under the divine guarantee, along with the whole of our 'natural inclination to believe'; but the reliability of all such evidence is strictly subject to the exercise, on our part, of due care and discrimination. Its reliability depends in the first instance upon reason, and ultimately, I suppose, upon the integrity of the will, which is necessary even for the operations of reason. There is not the least suggestion in Descartes of the insane view that faith can enable the human intelligence to dispense with all safeguards and neglect all rules for the proper exercise of its powers. On the contrary the principle is that God 'has permitted no falsity in my opinions which he has not likewise given me a faculty of correcting'.² There seems therefore no reason why even the unreliable faculty of memory should not be certified by God in this sense, namely that it is not *incorrigibly* unreliable; and I have little doubt that Descartes believed our confidence in all human faculties to depend ultimately upon our assurance of God's will for truth and goodness. Hence it seems to me that there is nothing intrinsically absurd, or even improbable, in the view we are considering. Descartes might very well have regarded memory as the main element of uncertainty which he had to eliminate; he might have held that knowledge of God could remove this uncertainty; and perhaps it would not be unfair to suggest that he ought to have noticed this possible source of doubt. All this, however, is beside my point. The question is: Was it in fact the fallibility of memory which Descartes had in mind as the source of his doubt concerning past intuitions? And I suggest that, on the available evidence, it was

¹ Mr. Keeling himself criticises Descartes severely for holding the view which Mr. Keeling has attributed to him; it is, he observes, 'indefensible', and this 'for empirical reasons' (*op. cit.*, p. 248).

² *Med.* VI (A.T. VII., p. 80). Cf. *Med.* IV (A.T. VII., p. 54): 'It is likewise certain that he has not given me a faculty which will ever lead me into error, *provided I use it aright*'. (My italics.)

not; that Descartes's statements on this subject all consistently indicate a doubt which affects in some way or other the faculty of clear and distinct perception itself; and that consequently, notwithstanding the vindication of present intuitions, it is this faculty which is supposed still to require some kind of certification.¹

Let us now examine Descartes's own statements on the subject. I will divide the material by considering two closely related questions: (1) How does Descartes describe the doubt which made it necessary for him to prove the existence of God? And (2) How does Descartes describe the assurance which he gains by means of this proof?

- (1) *How does Descartes describe the doubt which made it necessary for him to prove the existence of God?*

As I have already stated, we may take it to be agreed that the doubt in question is supposed to affect only past, or remembered, clear and distinct perceptions; that is to say, it arises only when we recall that *we have had* certain such perceptions, without actually reinstating them. What we have to ask is: Precisely how does this doubt apply? Am I supposed to doubt the fact that I had such perceptions, or is there some other ground for doubt, which would remain even if this historical fact were taken to be established?

We may put the question in more detail. We are told that the doubt arises only when I cease to attend to the evidence which led me to believe any particular proposition, and begin to reflect upon the reliability of evidence in general and upon my own origin and credentials; in particular, when I consider the possibility that I may have been created by some absolutely omnipotent being who is not the good God I have been taught to believe in, but a *mauvais génie*, a father of lies. Now what exactly is it that this *mauvais génie* is suspected of doing, or of having done, to prevent me from attaining to truth?

If the theory I am criticising were correct, we should expect the answer to be that he has given me an incorrigibly defective memory. The supposition that he has done so is, in fact, included

¹ In the *Regulae*, it is true, Descartes states quite clearly that one difference between intuition and deduction is that the certitude of the latter depends in a way upon memory; and that since memory is notoriously fallible, deduction is open to a doubt which does not affect intuition. This doubt, however, is to be dealt with (he says) by the practice of certain obvious safeguards, such as the use of writing, and the habit of constantly running over the various steps of a proof so as to leave as little as possible to the hazards of memory. There is no suggestion that knowledge of God is necessary; and Descartes's whole manner of expressing himself here is so different (and so unambiguous) that I cannot believe that *this* doubt is identical with the doubt which interrupts the *Meditations*. See A.T.X., pp. 370, 408-409, 454-455; and cf. the statement to Burman (below, p. 213, n. 1), which suggests that the need for God's guarantee of memory simply did not occur to him in the context of the argument about the circle.

in the extreme sceptical hypothesis of the *First Meditation* (as summarised at the beginning of the *Second*), but not in the required context. The *mauvais génie* is here supposed to cause me to have ideas (sensations and memory-images) to which no realities correspond or ever corresponded; but the doubt arising from this supposition affects my 'natural inclination' to believe in such correspondence, and does not touch the operations of pure intellection—a point which Descartes himself makes clear at the time. Wherever Descartes is considering the relevant issue—the possibility of doubting past clear and distinct perceptions—he expresses himself in quite a different way, and his expressions are consistent, indeed almost identical, in all the important passages. Let us see what he says.

The main statement is, of course, in the *Third Meditation*:¹ and here, speaking of simple mathematical propositions, he tells us that 'if I afterwards judged that we ought to doubt of these things, it was *for no other reason* than because it occurred to me that a God might perhaps *have given me such a nature as that I should be deceived, even respecting the matters that appeared to me the most evidently true*'. In the next sentence he repeats, in very similar words, his belief that God might 'cause me to err *even in matters where I think I possess the highest evidence*'.² And when we turn to the end of the *Fifth Meditation*,³ we find the point made with regard to mathematical demonstrations, thus: 'as soon as I cease from attending to the process of proof, *although I still remember that I had a clear comprehension of it*, yet I may readily come to doubt of the truth demonstrated, if I do not know that there is a God; for I may persuade myself that *I have been so constituted by nature as to be deceived even in matters which I think I apprehend with the greatest evidence and certitude, especially when I recollect that I frequently considered many things to be true and certain which other reasons afterwards constrained me to reckon as wholly false*'.

With these statements in the *Meditations* we may compare the parallel passage in the *Principles*,⁴ where the doubt is said to arise in the mind when it ceases to attend to the premises from which a given conclusion has been deduced and recalls the possibility '*that it has been created of a nature that is liable to be deceived, even in what appears most evident*'. The same formula is repeated later in the book,⁵ and is also used in the *Replies to Objections*⁶ to show reason why an atheist cannot attain to mathematical science. These last passages were written, it should be noted, *after* Descartes had become aware that his argument might be accused of circularity; when he was

¹ A.T. VII., p. 36. I use Veitch's translation where available and have italicised the important words in each quotation.

² '*etiam in iis quae me puto mentis oculis quam evidentissime intueri*'.

³ A.T., VII., p. 70

⁴ I., 13 (A.T., VIII., pp. 9-10).

⁵ I., 30 (A.T., VIII., p. 16).

⁶ *Ilae Resp.* (A.T., VII., p. 141); cf. *IVae Resp.* (A.T., VII., p. 428).

therefore fully on his guard and was taking particular care to avoid misunderstanding.

Now I do not deny that there are difficulties in these statements; but if Descartes's intention is to tell us that his only ground for doubt is the inaccuracy of memory, I suggest that his manner of expressing himself is unpardonably and incredibly obscure. There is not one word of the inaccuracy of memory either in the passages I have quoted or, so far as I know, in any other statement which he makes in the connexion. There is one regular formula which is repeated again and again, even after (according to the theory I criticise) it has been grossly misunderstood: the ground of uncertainty is consistently represented as a doubt whether I may perhaps have a nature which is capable of error even in things which seem most evident. This phrase, admittedly, does not define precisely the kind of error which Descartes had in mind; but I submit that the only natural interpretation is to take it as questioning not the accuracy of memory-judgments but the worth of clear and distinct perception itself. It is (I presume) agreed that the words 'things which seem most evident' cannot refer to or include memory-judgments but denote the intuitions of the understanding;¹ the only question is how I am supposed to conceive such an error to arise. It is true, no doubt, that if I supposed my nature to include an incurably delusive memory, this might be spoken of as a possible cause of error regarding my clear and distinct perceptions, in the sense that I might then think that I had experienced them when I had not; but if this is the sense which Descartes intends, I do not see how he could have imagined that he had made his meaning clear. I would draw attention especially to the passage quoted from the *Fifth Meditation*, with its explicit mention of the memory-judgment ('although I still remember,' etc.); these words would naturally be taken to be assuming a *correct* memory-judgment, in spite of which there remains ground for doubt.

I do not claim, however, that these passages are decisive, but only that it is natural to take them as meaning what they seem to mean, namely that in the conditions specified I am supposed to feel misgiving not about my memory but about my power of understanding. I do claim that there is decisive confirmation in the answer to our second question.

(2) *How does Descartes describe the assurance which I gain through the proof of God's existence?*

In the *Fifth Meditation*² the ground of my assurance is stated to be that, after proving God's existence and veracity, I can thence infer 'that all which I clearly and distinctly perceive is of necessity true';

¹ Notice the very explicit phrase quoted above, p. 210, n. 2, *mentis oculis quam evidentissime intueri*.

² A.T., VII., p. 70.

so that 'although I no longer attend to the grounds of a judgment, no opposite reason can be alleged sufficient to lead me to doubt of its truth, provided only I remember that I once possessed a clear and distinct comprehension of it'. It will be noticed that what I am supposed to infer from my knowledge of God is definitely said to be a guarantee of my clear and distinct perceptions, not of the accuracy of my memory; and that *once I have this guarantee*, all that is necessary is that I should remember that I have had such a perception. Here again it is natural to understand that a *correct* memory-judgment is being assumed, and most unnatural to suppose that the 'opposite reason' which Descartes contemplates is the inaccuracy of memory; especially in view of the fact that he goes on to suggest three 'opposite reasons' which might occur, among which the only one which is really applicable is said to be the possibility that 'my nature is such that I may be frequently deceived'—the possibility which can now be excluded as a result of my knowledge of God. Inaccuracy of memory is not mentioned at all.

If, however, this passage as it stands may still be thought ambiguous, the ambiguity is cleared up in a further discussion of the point in the *Replies to Objections*.¹ Here Descartes states explicitly that the doubt regarding past intuitions is removed when we have such a knowledge of God that we are aware that *the faculty of understanding* given by him must tend towards truth. This, he says, is the point which he has clearly explained at the end of the *Fifth Meditation* (the passage just quoted); so clearly indeed (in his opinion) that he does not see that there is anything to add to it.

The parallel statement in the *Principles*² seems equally conclusive. Here he says: 'Whence [i.e., from the veracity of God] it follows that *the light of nature, or faculty of knowledge* given us by God, can never attain to any object which is not true in so far as it is attained to by that faculty, that is, in so far as it is clearly and distinctly apprehended. For God would have merited the name of deceiver if he had given us *this faculty* perverted and such as to take falsity for truth. *Thus that ultimate doubt is removed, which arose from our ignorance as to whether perhaps our nature was such that we might be deceived even in those things that appear to us the most evident.*'

I do not see how we could be told more plainly that it is the faculty of understanding itself which was the original object of suspicion and which is now supposed to be guaranteed by God. If any further corroboration were needed, we might observe how Descartes responded when, in this context, the question of memory was actually brought up. In the *Conversation with Burman*³ it is suggested to Descartes that, even though the doubt of *ingenium* is dispelled by the proof of God's existence, we may still distrust *memoria*. This is clearly put forward as a new point, by way of

¹ *Ilae Resp.* (A.T., VII., p. 144; cf. p. 146).

² I., 30 (A.T., VIII., p. 16). I have slightly modified Veitch's translation.

³ A.T., V., p. 148.

criticism of Descartes's argument (which is taken to be as I have maintained); and it is noteworthy that Descartes replies, *not by a further appeal to God's veracity*, but by the recommendation that we should keep a check upon our memories by the use of 'writing and other such means'.¹ If the uncertainty of memory were the very point which Descartes has all along had in mind, this passage becomes wholly unintelligible.

To sum up. From the evidence which I have quoted there seems to me to be only one possible conclusion: namely that, although Descartes might well have included the question of the accuracy of memory in his statement of the ultimate doubt, it in fact never occurred to him to do so. The suspicion is always directed upon the faculty of clear and distinct perception; so far, that is, as its validity is considered *in general*, or in abstraction from any actual instance of its operation. And although there may be, somewhere in Descartes's voluminous writings, passages in another sense, I question if these could undermine the testimony of the *Meditations*, the *Replies to Objections*, and the *Principles*.²

Accepting this conclusion, what are we to make of Descartes's position? Is his reply to the charge of circularity valid, or even in any degree plausible? I do not know how these questions should be answered, and I am content to leave further speculation to the friends of Cartesianism. I would suggest to them only this: that however anxious we may be to acquit Descartes of a gross error of reasoning, it is yet easier to believe that he committed such an error than to believe that he completely failed to make clear to his readers a relatively simple point—which, it may be noted, he had expressed without difficulty in the *Regulae*—and that he failed not once or twice, but over and over again, and in every single passage in which he made the attempt. The clarity of Descartes's writing can be too highly rated, but there are limits to the degree of literary incapacity which I should care to attribute to him.

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¹ *De memoria nihil dicere possum, cum hoc quisque apud se experiri debeat an bene recordetur; et si de eo dubitat, opus est scriptione et similibus quae ipsum juvent*. As in the *Regulae* (see p. 209, n. 1).

² I have not quoted from the *Discourse*, since I have M. Gilson's assurance that Descartes '*n'y dit pas un seul mot de la mémoire, mais y insiste, au contraire, sur le fait que Dieu seul garantit la vérité de nos idées claires et distinctes*'.

KANT'S SO-CALLED COPERNICAN REVOLUTION.

It has become a commonplace among English interpreters of Kant (to say nothing of those whose knowledge of him is at one or more stages removed from the study of his writings) to refer to the phrase 'the Copernican Revolution' as though the words actually occurred in Kant. The analogy Kant is commonly supposed to have drawn between the revolution which he himself achieved in the realm of philosophy and that which Copernicus effected in the traditional theory of the Universe has, moreover, sometimes been held to be of sufficient importance to assist the student of Kant in deciding between conflicting interpretations of the doctrines of the *Critique*. It may, therefore, come to some as a surprise to have serious doubts raised as to whether Kant ever used the phrase.¹ Yet it certainly does not occur in the *Critique of Pure Reason*; and I have searched for it in vain in indices to the other *Critiques*, as well as in several other volumes in F. Meiner's indexed edition of Kant. I have, in fact, not succeeded in finding anywhere in Kant a reference to Copernicus except the two well-known occurrences in the *Vorrede* to the second edition of the *Critique of Pure Reason* (first added in 1787).²

I think it is sufficiently clear that the origin of the expression must be sought in the *Vorrede*. Unless I am mistaken the error goes back to the use of the word 'Revolution' in the first sentence of the paragraph which begins at the bottom of B. xv., *i.e.*, the paragraph in which the former reference to Copernicus occurs. But the discussion which has preceded this paragraph leaves no doubt that the 'Revolution' here referred to has nothing to do with Copernicus. The structure of the *Vorrede* to the second edition, which follows that of the *Prolegomena*, is based on the affinities and differences between the three branches of knowledge—mathematics, physics (*Physik* or *Naturwissenschaft*), and metaphysics. Kant maintains that each of the two former, after a preliminary period of groping (*Herumtappen*) suddenly became constituted a science, entering upon the *sicheren Gang einer Wissenschaft* in consequence of a 'Revolution'.

¹ Kant does not appear to use the expression 'Copernican hypothesis' either. Where Dr. K. Smith in his translation of the *Critique* (p. 22) renders *mit den ersten Gedanken des Copernicus* as "Copernicus' primary hypothesis," he adds the German. But from his *Commentary*, pp. 19, 22, the unsuspecting reader might conclude that Kant used the expression.

² I am informed that a search in R. Eisler's *Kant-Lexikon* failed to reveal further references to Copernicus.

He first deals with mathematics. Here (in B. xi.) the word 'Revolution' is italicised ('*gesperrt*'), and is thus shown to be (what would apart from any such emphasis be sufficiently clear) the key idea of the paragraph. Who it was who effected *this* revolution, whether it was Thales or some other unknown person upon whom there broke a light (*ging ein Licht auf*), Kant will not say. In the case of physics, the world had to wait much longer for a corresponding revolution. It was only some century and a half ago that Bacon of Verulam "by his ingenious proposals, partly initiated this discovery, partly inspired fresh vigour into those who were already on the way to it"; and it was "when Galileo caused balls, the weights of which he had himself previously determined, to roll down an inclined plane; when Torricelli made the air carry a weight which he had calculated beforehand to be equal to that of a definite volume of water; or in more recent times when Stahl changed metal into lime and lime back into metal by withdrawing something then restoring it, [that] a light broke upon all students of nature (*so ging allen Naturforschern ein Licht auf*)" (B. xii. f.). This latter quotation puts it beyond question that when at B. xv., f., Kant refers back to "the examples of mathematics and natural science, which by a single and sudden revolution have become what they now are," the revolution in natural science concerned has nothing to do with Copernicus and his heliocentric doctrine, but that it is the successful foundation of experimental physics in the seventeenth century by such men as Galileo, Torricelli, and Stahl to which Kant alludes.

In B. xiv. f., the question Kant asks is whether a change in the method of metaphysics, corresponding to these two revolutions in mathematics and natural science respectively, might not put an end to its mere groping (*cf.* B. xv., *Herumtappen*) and lead to more successful results than hitherto. Whether this is so can here too only be discovered by trial (*Man versuche es daher einmal*, etc., B. xvi.). To make trial of an alternative hypothesis is to follow Copernicus who when he found he could not achieve satisfactory results by assuming one hypothesis, made trial of (*versuchte*) another. Now in metaphysics, as far as the intuition of objects is concerned, it is possible to make trial similarly (*auf ähnliche Weise versuchen*). Such is Kant's argument. The main reason (I am convinced) why Copernicus is introduced into the argument is merely to illustrate the propriety and possible fruitfulness of making trial of an untested hypothesis, the occurrence of the verb *versuchen*, in each of the three successive sentences making it clear that this is the *tertium comparationis*. Any further parallelism between Copernicus and himself is only incidental to his line of thought.

That this is the correct interpretation is borne out also by the fact that while Kant uses the word *drehen* in regard to Copernicus' theory of planetary motion, in the next sentence but one (on B. xvii.), where he comes to refer to his own theory he uses *richten*.

A study of the *Vorrede* shows that all the way through in the analogies which Kant draws he brings out correspondences by the use of identical terminology. Had he introduced the Copernican astronomical doctrine primarily because he regarded it as a counterpart of his own theory of knowledge, he would undoubtedly have made the terminology of his analogy correspond.

Confirmation of this interpretation of the passage is to be found in the discussion of the use of a hypothesis to be found in Wolff's *Elementa Matheseos Universae*.¹ In Kant's days it was obligatory on professors to take a compendium as the basis of their lectures; and on at any rate one of the two occasions on which Kant lectured on mechanics (in 1759-60) he used for this purpose² Wolff's *Elementa Mechanicae*, i.e., part of the *Elementa Matheseos Universae*. Appended to this whole work is a long dissertation on scientific method, the 'Commentatio de Studio Matheseos Recte Instituendo,' and it may therefore be taken for granted that Kant would have been very familiar with this. Here (in §§ 309-311) there is a discussion on the very point—the usefulness of hypothesis as a means of scientific progress—and the example given is that of Copernicus and Kepler, with the subsequent verification. Hence it would seem very likely indeed that the reference to Copernicus was introduced with this passage in mind.

Whether or not Kant was also familiar with Copernicus' *De Revolutionibus* itself I have been unable to discover; but in spite of Dr. Kemp Smith's belief that he was, I am inclined to doubt this. I think that if Kant had studied the treatise at first hand there would be more plentiful references to both Copernicus and the *De Revolutionibus* in his writings than there appear to be, though those familiar with the byeways of Kant's writings may be able to put me right on this point.

If, however, Dr. Kemp Smith is right in his belief and Kant was really acquainted with it, there may be an allusion in B xxii., n. to a certain parallelism between the *De Revolutionibus* and the *Kritik*, the discovery of which would certainly have been welcome to Kant's sense of architectonic propriety. In that footnote, Kant states that what he is developing purely hypothetically in the *Vorrede* will be established "apodeictically not hypothetically" in the body of the *Kritik*. And there is a very similar relation between the preface to the *De Revolutionibus* and the treatise itself, for whereas in the preface the Copernican doctrine is asserted to be only a hypothesis, in the body of the work its truth is taken for granted.³

¹ Geneva, 5 vols., 1746.

² I owe this information to E. Adickes, *Kant als Naturforscher*, i., 11 n.

³ It might not be quite impossible to see a reference to the difference in standpoint between Copernicus' preface and the rest of the *De Revolutionibus* in the puzzling *mit den ersten Gedanken* [Erdmann conjectures *mit dem ersten Gedanken*] on B xvi., and also the *anfänglich* on B xxii. n. Dr. Klibansky, who has kindly furnished me with a detailed exegesis of

If this is so, it must be presumed that Kant was not conscious of the circumstances to which the preface to the *De Revolutionibus* owed its origin, though they were easily discoverable to the historians of science in his day.¹ (Even the erudite Dr. Kemp Smith was not familiar with them when he wrote the relevant pages in his *Commentary*.) Fearing the censure the heliocentric doctrine would meet with at the hands of the ecclesiastical authorities, Copernicus long delayed its publication and it was only at the very end of his life that he allowed it to appear. But Andreas Osiander, the Protestant theologian to whose care it was entrusted, managed to slip in the Preface, and it is only here that the hypothetical nature of the heliocentric theory is asserted. The Preface, incidentally, achieved its purpose in so far that the work was not put on the Index until the time of the Galileo affair in 1616 (where it remained until 1758). Copernicus himself certainly did not conceive of his doctrine as a 'hypothesis'. It may be added that the sub-title of the *De Revolutionibus*, in which the phrase *et novis insuper ac admirabilibus hypothesibus ornatos* occurs that contributed so much to the belief that Copernicus was arguing only hypothetically, was also due to Osiander.

I do not deny that when Kant had once introduced Copernicus into his discussions, he was struck by the analogy that existed between Copernicus' attempt to explain the apparent motions of the heavenly bodies by the change in position of the observer and his own transformation in his mode of thinking. So much is clear from B. xxii. n. I am satisfied if I have proved (i) that, at any rate in his best-known works, Kant never spoke of a 'Copernican Revolution,' (ii) that he was never concerned to stress the similarity between his own teachings and the doctrines of the *De Revolutionibus*, and (iii) that in the *Vorrede*, with its elaborately worked out historical parallels, the reference to Copernicus at B. xvii. does not stand in any immediate relation to the main line of the argument.

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the two passages, does not give any countenance to this interpretation. Dr. Kemp Smith's rendering of B. xvi., "Copernicus' primary hypothesis," apparently takes the *ersten* as referring to logical, not temporal, priority.

¹ The facts were clearly set out, e.g., by P. Gassendi in his life of Copernicus appended to his *Tychonis Braheii . . . Vita*, p. 36 (Paris, 1654).

THE PARADOXES OF LOGIC.

IN his article entitled *Les paradoxes de la logique*, in vol. xlv., N.S., No. 178, of *MIND*, Mr. Perelman gives an apparently new analysis of some logical paradoxes which is stated with remarkable perspicuity. However, the criticism of Mr. Grelling in his reply, *The Logical Paradoxes*, in No. 180, shows that this analysis need not in every respect be regarded as adequate and satisfactory.

Anyone who—like myself—has for a long period of time been concerned with these problems cannot help admitting that Mr. Grelling's comparison with the egg of Columbus appears indeed to apply in an almost astounding manner. The egg which Columbus set up on end was not the same intact one, which had been handed to him, but one which had been somewhat flattened. Therefore the problem actually solved by Columbus was not the one meant by his hearers but a somewhat different one. As a matter of fact, it would seem that Mr. Perelman in a certain sense elucidates the paradoxes treated by him so that an objection can hardly be raised, but nevertheless not quite in the sense in which they are really meant. Thus the reader retains a feeling of dissatisfaction just such as the hearers of Columbus may have experienced.

As regards the choice of the paradoxes dealt with, I agree with Mr. Grelling that we can do without the so-called paradox of the barber and the paradox of relations; in the latter case, however, for a different reason. When we consider that the free variable (or constant) *S* actually does not play a part in the latter paradox and accordingly eliminate it, we come apparently back to Russell's paradox. The paradox of relations is therefore nothing more than Russell's paradox in a somewhat complicated form. On the other hand, although the elimination of unessential complications is justified and reasonable, nevertheless simplicity as such must not be made the basis of selection. One misses the paradoxes of the class of things and of transfinite cardinal and ordinal numbers. It cannot be legitimately objected that these paradoxes do not belong to elementary logic but rather to the theory of aggregates. For just as the paradoxes of the theory of aggregates cannot be satisfactorily solved and excluded—as mathematicians have often tried to do—without explicitly taking account of those of elementary logic also, likewise a solution proposed for these paradoxes cannot be regarded as adequate, if it does not apply equally to those of the other kind. How far this is true for Mr. Perelman's analysis must still be regarded as doubtful.

According to Mr. Perelman, the paradoxes considered by him are of such a nature that from a given premise conclusions are drawn in agreement with the acknowledged rules of inference and that these conclusions, being contradictory to each other, prove the said premise to be false in accordance with the principle of "*reductio ad absurdum*". Now it is true that each of the paradoxes in question may be put in a form of this kind or—speaking more cautiously—may be reconstructed in such a form. But in the form of the paradoxes acknowledged as authentic the true point of issue is not a premise, *i.e.*, an assertion, but a *definition*. Thus the contradiction arises in conformity with the laws of logic without any additional assumption merely by the occurrence of a certain definition.

To the last statement I must, however, make several reservations. Firstly, I do not assert it for all paradoxes of logic in general, but explicitly for such as are treated by Mr. Perelman. Secondly, the statement is essentially concerned with the representation of the paradoxes in symbolic form, not with their formulation in language as such. *E.g.*, Russell's paradox may be given the brief formulation in language: "Does the quality 'not applying to itself' apply to itself or not?"—where neither a premise nor a definition is to be found. But if one attempts a formulation in symbols, it is impossible to do without the one or the other. And, thirdly, it is not necessary that the said definition should explicitly occur in the formulation of the paradox, but instead, of course, an accepted notion may occur, the definition of which is tacitly assumed as correct.

It will be sufficient to explain what I mean by means of Russell's paradox. This paradox, as its author undoubtedly intended, is represented symbolically in the following way: Starting from the definition $F(\phi) = \sim \phi(\phi)$ Df, we get the universal equivalence $\phi \cdot F(\phi) \equiv \sim \phi(\phi)$ and, substituting for ϕ the quality F , we derive the contradiction $F(F) \equiv \sim F(F)$. According to Mr. Perelman, we should start from the equivalence rather than from the definition.

Now it may be argued that it cannot make any serious difference whether we start from the definition or from the equivalence, considering that we might legitimately pass from the equivalence to the definition, making use of the fact expressed by the equivalence in order to define F . This is so far perfectly true. But, as to the practical consequences, this very difference, unimportant as it may seem, gives rise to a problem of a quite different bearing and difficulty. That deciding the truth or falsehood of an *assertion* may be very difficult and even hopeless, is a fact to which we are perfectly accustomed and which we regard as unavoidable. Taking as point of issue the *definition*, we have a quite different state of things. Provided a contradiction is caused merely by the occurrence of a definition, and provided we have a right to regard the inferences drawn and the system of logic in general as correct, we are compelled, it is true, by reason itself to declare the definition in question as

inadmissible or, at best, admissible only in an adequately restricted sense. But we cannot, in practice, make it a general condition for establishing any definition whatever, *i.e.*, for a mere act of denomination, that there must not exist any chain of reasoning—however long and complicated—leading to a contradiction by the use of the said definition. Such a condition, however possible theoretically, would be quite intolerable in the practice of logic and mathematics.

Accordingly, from the point of view of exact logic, *the very essence of the problem of paradoxes is no more nor less than the problem how to state and to apply symbolic definitions correctly, more generally, how to decide whether a given expression can be symbolically substituted in a given expression.* (This generalisation, too, is essential for certain forms of paradox.) It is solely because of this state of things that the paradoxes embody or rather contain any longer a problem for exact logic; apart from this, they would be thoroughly settled from the standpoint of that science. It would seem that Mr. Perelman does not penetrate at all to this essence of the problem.

It is not possible in the present connection to explain more fully, and to formulate and to treat exactly, the problem indicated above, nor is it necessary, as I may refer for this purpose to my own article, *Zu den Widersprüchen der Logik und der Mengenlehre*, (Jahresbericht der Deutschen Mathematiker-Vereinigung, vol. xl., 1931), where the said task is performed sufficiently to make criticism possible.

The starting-point of my own analysis of the paradoxes is this: Whereas, in any logically correct form of argument, it must be possible to replace the notions and symbols introduced by definition by those complex terms for which they stand, this process of translation back into the original terms fails for the symbolic formulation of Russell's paradox, especially for the complex $F(F)$. (F. P. Ramsey wrote me on this point: "This is a striking fact which I, at least, had never realised.") Thus the definition in question turns out to be more essential to the argument of the paradox than—being a mere definition—it ought to be. The fact that it is of essential importance for the general problem explicitly to consider also the genuine paradoxes of the theory of aggregates, strikingly appears from the paradox of the class of things. (Let T be the class of things in general, C the class of classes in general. Being a sub-class of T , C cannot have a greater cardinal number than T ; but, since C is the class of sub-classes of T , it must, by Cantor's theorem—which asserts that the class of sub-classes of a given class has always a greater cardinal number than the class itself, have a cardinal number greater than that of T .) As the analysis of the paradox shows, in this case the contradiction does not occur owing to the use—or rather abuse—of a definition, but in fact owing to a pre-assumed assertion, namely that of Cantor's theorem. It also appears independently of the argument of the paradox that the said theorem is not strictly true in general; it must be understood, however, that the failure of this premise cannot be made so perspicuous as in the cases considered by Mr. Perelman.

The theory set forth in my article, though it retains the distinction of logical types as such, which is perfectly natural and justified, nevertheless discards the *limitations* involved by Russell's simple or ramified theory of logical types; in particular, the types are not supposed to be mutually exclusive. (*E.g.*, we have the type of things—which is the most comprehensive one—the type of qualities and the type of qualities of qualities, the second being a sub-class of the first and the third of the second.) Notwithstanding, an occasional limitation of these types, which has proved to be natural and unavoidable, must be taken into consideration.

How the so-called syntactic paradoxes can be solved by my principle, is shown by applying it to the paradox of the liar; the solution of this paradox may be extended without much trouble to the other paradoxes of the same group. (It may be that, in order to state exactly in symbols these paradoxes and their solution, a distinction between several symbolic languages will be necessary.)

Though I do not claim to be a Columbus or Oedipus in exact logic, yet I am convinced that I have shown the possibility of and the justification for a conception of our problem that should not be passed over in silence in future discussions.

HEINRICH BEHMANN.

VI.—CRITICAL NOTICES.

A Study in Plato. By W. F. R. HARDIE. Oxford: Clarendon Press, 1936. Pp. xiii + 171. 8s. 6d.

MR. HARDIE'S book is one which I find it a little difficult to review, and that for two reasons. The lesser reason of the two is a merely personal one, that much of it turns out to be a sustained polemic against certain views of Prof. Burnet and myself, and it is, of course, difficult for me to feel sure that, with the utmost goodwill in the world, I have succeeded in so completely avoiding 'partiality to self' as to appreciate all the writer's points at their full worth. The more serious reason is that there is a certain want of unity about the book itself. It has, to all appearance, a main thesis, but this thesis is, at least, in abeyance through nearly half the work, which is, in fact, not so much a study of *Plato* as a number of studies of problems raised by the Platonic dialogues. It is quite possible not to be in the least convinced by the author's main contention, that the first 'hypothesis' of the second part of the *Parmenides* is seriously meant by Plato as a statement of the supreme principle of a 'transcendence' philosophy which is his own—in fact, that Plato was himself something like a 'Neoplatonist'—and yet to agree with much or most of what Mr. Hardie has to say about, *e.g.*, mathematical science, or the 'divided line,' or the 'Socratic dialectic'. And again, it would be equally possible, I suppose, to accept what I have, I hope not incorrectly, called Mr. Hardie's main thesis and yet to feel that his way of correcting Plato's 'errors' about the nature of 'opinion', or of disposing of the Ferguson-Stocks exposition of the 'divided line', is a little too summary to be quite convincing. Some of the issues raised in the first half of the not very lengthy book are perhaps too intricate to be disposed of without much more examination than the author has apparently thought needful.

I would suggest also that Mr. Hardie seems to be hampered in the treatment of Plato by certain constitutional 'imperfect sympathies'. I suspect him of being constitutionally not very sympathetic with humour, and not always well-qualified to pronounce off-hand on the question whether the utterances of a Platonic character are to be taken *au grand sérieux*. He tends a little, I fear, to see humour only when it approximates to knock-about farce. And his confidence that Plato does not mean his Theism or his profession of conviction about human immortality to be taken altogether in earnest is a

little discounted by his own unconcealed and rather irrelevant want of belief in both doctrines. I feel sure that he is anxious to under-rate their significance for Plato from a certain personal dislike of them. And if I may say so, I think he suffers further from a defect very incident to youth, a tendency described by himself as 'dogmatism', when he meets it in others. Plato's convictions are too often brushed aside, in cases where I at least feel that there is a good deal more to be said for them, on the strength of some curtly enunciated thesis in current philosophy which might itself be fairly considered dubious, or even on the mere ground that some philosopher or other has not concurred in them; the greatest of all writers on philosophy is, at times, treated too much like a Fourth Form boy who has shown up a 'Latin prose' for correction. It is a minor exhibition of the same disposition that Burnet and others of us should so often be reprimanded on the ground that our attempts to interpret Plato are 'conjectural', as though we were not perfectly well aware of the fact, or as though Mr. Hardie's rival interpretations were not necessarily equally conjectural (or is it meant that our conjectures are bad ones, but Mr. Hardie's good? That is a perfectly sound line to take, but it has to be taken by making the badness of our conjectures and the goodness of his apparent, not by writing as though all the conjecturing came from one party to the discussion.)

I do not mean, of course, that Mr. Hardie has not a case to offer for his views, and that sometimes the case presented is a good one and deserves to be carefully examined on its merits, nor that, in the long polemic which he develops against myself, he does not aim some shafts which go home. I think he does these things, and I have found him always interesting and often acute, though perhaps from 'partiality to self', I do not feel satisfied that he is quite in the position to sing *τῆνέλλα καλλίνικος*.

In the brief introductory discussion of the extent to which the Platonic dialogues may be taken as stating the personal convictions of their author, there is little on which Mr. Hardie and myself would not be agreed. But I should doubt the legitimacy of starting (p. 3) by including the *Theætetus* and *Parmenides* along with the *Sophistes*, etc., in a single group of 'later' dialogues assumed to present a uniform doctrine. The linguistic evidence afforded by the sudden adoption in the *Sophistes* of the Isocratean trick of avoiding *hiatus* seems to me in favour of Burnet's view that the dialogue is separated by an interval of silence from all that had gone before it, and that we must not therefore assume that there has been no development of thought between the composition, e.g., of *Theætetus* and of *Sophistes*. (And if Mr. Hardie really means in certain later passages to assume that the *Parmenides* was written before the *Theætetus*, that again seems to me a doubtful 'conjecture'.) And while I fully agree with the remark on page 4 about the significant difference between the attitudes of the *Republic* and the *Sophistes* to *δόξα*, it

seems to me a mere mistranslation to render the definition of δόξα in the latter dialogue (διανοίας ἀποτελείτησις) by 'completion of intelligence'. The words mean only 'termination of a process of thinking'. The thinking is conceived as a debate carried on within the mind of the individual, and the δόξα as the 'decision' in which the disputing parties end by concurring. As to what is said on page 5 ff. (when the 'historical Socrates' is dragged in), I do not think there is any very great difference in principle between Mr. Hardie and Burnet and myself. On the point that Socrates also held that certain knowledge is knowledge of mathematical and moral standards, and that the pervasion of the universe by these standards is due to 'the good', we all seem to be agreed, and if so, the main theses of the *Phædo* and even the *Republic* are genuinely Socratic. Mr. Hardie's real opponents, whom he does not name, are the contemptuous rejectors of Aristotle's evidence about Platonism, such as Prof. Shorey, and, more recently, Mr. Cherniss, and as against them, the only antagonists whom he does name, Burnet and myself, are on his side. The real question about which we and he might find ourselves at variance is one which he does not indicate, viz., Is the χωρισμός to which Aristotle objected in Platonism, taught in the *Phædo* or is it not? May it not be true of the Socrates of that dialogue that he 'does not separate the universal'?

On the rather perfunctory discussion of the primary meaning of εἶδος and ἰδέα I have nothing to say, except that it is not quite true (p. 11) that I 'overlook' the sense 'sort' or 'kind' as a 'primary meaning'. On the contrary, rightly or wrongly, I set myself in a special essay in my *Varia Socratica* to argue that this meaning is not primary, but derived, exactly as the meaning *genus* is not a primary but a derived sense of γένος. I believe I should be much better justified if I urged that Mr. Hardie has wholly 'overlooked' senses of εἶδος, the senses in which it has given rise, through the translation *species*, to the modern *épice* and *spice*, and again to *Idyll*, to say nothing of the old mathematical expression τετράγωνον εἶδος. What is said on page 12 of the argument for εἶδη based on the sciences, though true enough, does not seem to me to bring out what is most characteristic in Plato's reasoning. His view is not merely that stated here, that a sensible character of an x may be here to-day and gone to-morrow, but something much more radical, that what you perceive by the senses is never ' x characterised by ξ ,' but always ' x coming-to-be (or ceasing-to-be) characterised by ξ .' The '*pendulum never is at rest*'. If you admit that sense discloses x as actually characterised by ξ , through however short an interval, his 'argument from the sciences' is emasculated.

In the actual discussion which follows of Plato's theory of sensation, I think Mr. Hardie is led astray by the tendency to 'modernise'. He finds fault with Plato for calling sensible qualities κινήσεις, on the ground that it is manifestly absurd to say that, e.g., a colour is a movement. But the absurdity is only patent because, of course,

when Mr. Hardie says a 'motion', he means a *translation*, whereas by *κινήσεις* Plato means only 'processes of change', and the absurdity, whether there or not, is at least not, as the translation makes it appear, open and palpable. Leibniz, for example, would have agreed with Plato that the sensible character never remains wholly unchanged for the briefest period, though Leibniz would have been as clear as Mr. Hardie that it is not a translation.

In the discussion, which immediately follows, of the theory of sense-perception outlined in the *Theætetus*, as elsewhere in the course of the argument, Plato is, I fancy, treated a little unfairly on the strength of an unconscious substitution of the modern notion of the *physical* for what Plato is really talking about, the *sensible*. But if we are to appreciate the argument of the *Theætetus* against 'extreme Heracliteanism' fairly, we need to remember that the processes which the modern physicist calls *physical* are not sensible; their reality is indirectly inferred from the macroscopic 'data of sense'. To see the full force of Plato's reasoning, we might, *e.g.*, ask what is meant when it is said that a particle is moving with a uniform acceleration. It will not be true that the particle actually has any given velocity, constated by direct measurement, during any interval, however brief, of its movement (for if it had, it would not be uniformly accelerated). The real fact is not that the particle has the velocity v either during the interval $t_1 - t_2$, or for any time within this interval. It will be that during the interval $t_1 - t_2$ its velocity changes from v_1 to v_2 , and we shall further need to explain that what we really mean by calling, *e.g.*, the initial velocity 'at t_1 ' v_1 is that if we take the lesser interval $t_1 - t_{11}$, the velocity will, during this interval, be 'in the neighbourhood' of v_1 , and can be made to differ from v_1 as little as we please by taking $t_1 - t_{11}$ small enough. I may add that I regret that both in this discussion (at p. 23) and again at page 159 Mr. Hardie should appear to give some hesitating and uncertain support to Adam's fantastic views about the objects of the Platonic astronomy. It is true, to be sure, that what the Platonic astronomer calls the 'real motions' of a planet are not the 'sensible motions' which can be seen by the bodily eye. But you could say as much about Jeans or Eddington. They too are concerned with movements which they take to be physically real, but not to be 'sensible'. As Burnet asked long ago, if the Platonic astronomy is concerned with some imaginary 'ideal' heavenly bodies, why is Plato so anxious to correct the current views about the orbits and periods of the actual planets?

Mr. Hardie goes on to discuss Plato's views about *δόξα*, maintaining that it was illegitimate for him to argue, as he does in *Republic* V., from the difference between knowledge and 'opinion' to a corresponding difference in the appropriate objects of each. I do not myself see that there is the difficulty Mr. Hardie seems to find about holding at once the two theories (1) that certain things are *proprie* objects of *δόξα*, that is, that you can never have scientific knowledge

of them, cannot know them, as Roger Bacon might have said, *certitudinaliter*, but can at best have what Leibniz calls a 'moral assurance' about them, and (2) that certain other things may be *proprie* objects of knowledge, though a *given man* has only a 'true opinion' about them. The philosopher may *know* exactly what justice is, the 'man in the street' does not, and yet it is quite possible that even the 'man in the street' may, in his better moments, think that justice is what *Republic IV.* undertakes to prove that it is. Or again, Fermat asserted that he had a proof of his famous 'last theorem' which no later mathematician has apparently rediscovered. If Fermat really had such a proof, the theorem is, in its own nature, an object of *ἐπιστήμη*, yet the numerous mathematicians who enunciate it as something which is believed to be true, but of which no proof is known, have only *ἀληθὴς δόξα* on the matter. Hence it seems to me illegitimate to say with Mr. Hardie that Plato's view that certain *classes* of objects are only accessible to *δόξα* while others admit of being known is a 'confusion'.

I think likewise that, partly from a tacit confusion between the *physical* and the 'sensible', partly from preoccupation with certain current logical theories, less than justice is done to Plato in the pages on *mathematical science and the Forms*. Is it as questionable as is maintained on page 41 that nothing sensible is an *exact* instance of the properties studied by the mathematician? Clearly you cannot, e.g., visualise a cube or a sphere all at once. And what about a plane or a point? Or a logarithmic spiral? And what is meant when it is suggested, as an alternative to Plato's position, (p. 45) that 'straightness is only a concept *in our minds*' (*italics mine*)? If this means that straightness is something *of* which we think, Plato might probably reply that we have granted the point for which he is contending. If it is meant that straightness is an act of thinking, the assertion seems to be simply false. I own that I do not in the least understand this sort of language (used by Mr. Hardie without any explanation) about what is 'inside' or 'outside' the mind. And I cannot follow the argument of page 47 where Plato is supposed to damage his own position by admitting that the geometer 'makes use of' visible diagrams. For it is only said that geometers, in point of fact, did this, not that they necessarily must do so. In fact, the subsequent history of geometry, I should say, has demonstrated that the second statement would be false. Cook Wilson, indeed, as Mr. Hardie remarks, thought it true, but the consensus of the geometers (who ought to understand their own business) is against him, and I should like to invite Mr. Hardie to consider once more the weighty argumentation of Couturat on this very question (in the examination of the mathematical doctrines of Kant appended to *Les principes des mathématiques*). I should also like to record my very grave doubts about the attempted equation of the *νόθος λογισμός* by which *χώρα* is said to be apprehended in the *Timæus* with the *διάνοια* of *Republic VI.-VII.* (The latter is expressly said to include number-

theory, and I conceive it to be quite impossible that *that* should have been called by Plato 'bastard reasoning'.)

In the interesting discussion of the meaning of the 'divided line' of *Republic* VI. Mr. Hardie is, of course, opposing the novel interpretations of Profs. Ferguson and Stocks. My sympathies, I own, are on his side, and for the reason he gives, that Plato himself has expressly directed us, in a way which can only be evaded by mistranslation, to read the simile of the 'line' in the light of that of the 'cave.' But at the same time, it should, I think, have been recognised that Ferguson's detailed study requires much fuller examination than the size of the present volume permits, and that he is not to be disposed of in quite the summary fashion which seems to satisfy Mr. Hardie.

In the brief description of 'the Socratic Dialectic', there is only one point to which I would advert. Mr. Hardie seems puzzled about the description of dialectic in *Republic* 533c as 'destroying' the postulates of the mathematicians. I think his difficulty would disappear if he would reflect again on the character of the three 'postulates' which Plato selects as examples; they will all be found to be assumptions which mathematicians have been driven to question or to discard as they have become more philosophical. Or Mr. Hardie might reflect on Berkeley's penetrating criticism of the postulates of the fluxionists and infinitesimalists of his day, and the way in which the 'postulate' of infinitesimal moments or infinitesimal augments has long ago been 'destroyed'. In any case the question is not about 'axioms and definitions' (p. 69), but about *postulates*.

The discussion of the 'separation' (*χωρισμός*) of the Forms to which Aristotle took exception seems to me far too perfunctory. It omits altogether the question which is, to my mind, of primary importance, whether when Aristotle says that this mistake was *not* committed by Socrates, he means that, though found in Plato's later work, it does not affect the reasoning of the Socrates of the *Phædo*. And I should take exception to the assumption (p. 73) that to say that a Form is 'separate' means that there are no 'particulars which exemplify it'. No doubt Plato meant that no particulars fully and completely embody such a Form, but is there any reason to think that he believed in Forms which are not embodied at all in *some* particulars? Mr. Hardie seems to hold that there are 'universals without instances' in ethics, but that Plato went wrong in assuming that the same thing is true in geometry. For there may be no particular act which is perfectly just, and yet particular acts may be 'more or less just', but one line cannot be more or less straight than another; if it is not perfectly and exactly straight, it is simply *not* straight. But can this distinction be really sustained? Two lines may certainly have different *radii* of curvature, *i.e.*, one may be more curved or less curved than the other. And that which is less curved is *more* nearly straight in exactly the same sense in

which one of two acts, of which neither is perfectly just, can be said to be more just than the other. For the matter of that, I do not feel sure that when Plato speaks, in the relevant passage of *Republic* V., of the "many justs" each of which is pronounced at times to be unjust, he is thinking of 'particular acts' at all. May he not be thinking of various formulæ, like those given by Polemarchus or Thrasymachus, each professing to state what justice universal is, and each apparently holding good over a certain field but breaking down elsewhere? And as to the doctrine attributed on page 76 to Aristotle that 'determinate forms of a universal' are the 'only universals there are', whether this is a tenable doctrine or not, I can hardly believe that it was Aristotle's, since it would follow at once that the *Categories*, Aristotle's ultimate *genera*, are not really universals at all. To come to the central thesis of the book, which is expounded in two sections on (a) the criticism of Forms in the *Parmenides*, and (b) the 'enigma' of that dialogue (i.e. the interpretation of the 'antinomies' of its second part), Mr. Hardie holds the view that in some way the antinomies contain a rejoinder to the criticisms which have argued that the 'participation' of Forms by particulars is fatal to the assumed unity of the Form itself, and further that the solution is that behind the Forms which are 'participated', there is a strictly imparticipable and 'transcendent' principle, the 'One' of the 'first hypothesis', which is identical with the 'Form of good' of the *Republic*. His solution is thus on broadly Neoplatonic lines, like that of M. Wahl (to whose careful study of the dialogue, however, he does not refer). The argument is developed in a polemic partly against Burnet and partly against myself, which I may be excused for treating as lightly as I can. I would ask the reader, however, to take note of two points. Mr. Hardie charges me with not discussing the 'external evidence' (of Aristotle and others) for Plato's identification of the One with the Good. Now if I have not 'discussed' this point, the reason was that I never supposed anyone would doubt that Plato identified a principle which he called 'the One' with the Good. But whether *this* principle is identical with 'the One' as described in the first antinomy of the *Parmenides* is another matter, or to put it loosely, to believe in a One which is also the Good, is not in the least necessarily to believe in the Neoplatonic 'first hypothesis'. And the case stands thus. There are only two passages in Plato which seem to speak of a principle which is 'beyond being': one is the description of the *ἰδέα τοῦ ἀγαθοῦ* in the *Republic*, the other is the 'first hypothesis' of the *Parmenides*. In the second of these, the consequence is rigidly deduced that the 'transcendent' One cannot even without contradiction be said to be *One*; thus it is apparently a 'pseudo-concept'. The natural—if not the necessary—inference would be that the Plato who wrote the later dialogues no longer meant to maintain the kind of 'transcendence' ascribed to his supreme principle in the *Republic*. Has Mr. Hardie asked himself seriously what would be the result of applying the

methods of Plato's 'Parmenides' to the propositions that 'the One is the Good', or even that 'the One is an ἀρχή'? My other general observation shall be made more briefly. Mr. Hardie denies that I can be right in finding humour in the antinomies, because he does not see it there. But that may be no sufficient proof that it is not there. (Jowett, it will be remembered, equally missed the humour of the attack on valetudinarianism in *Republic* III. and was moved to read Plato a homily on the 'unchristian' character of his sentiments.) In particular, when great stress is laid on the declaration of 'Parmenides' that Socrates is suffering from an insufficient training in logic, we need to remember that it is very possible that the remark, put into the mouth of the master of Zeno, is meant to be double-edged, and is by no means to be taken at its 'face-value'. And in the reasoning by which (p. 56) Mr. Hardie attempts to prove that Proclus was wrong in his contention that the objections urged against 'Socrates' by 'Parmenides' are fallacious, I think he has overlooked the important point that it is 'Parmenides' who silently substitutes ὁμοιον εἶναι for the ὁμοίωμα εἶναι of 'Socrates' (*Parm.* 132e), either not seeing or hoping that 'Socrates' will not see the important difference of sense between the two expressions. May not one say that such controversial methods, to borrow Mr. Hardie's phraseology, "will not do"? (And à propos of Mr. Hardie's comments on a discussion of this part of the dialogue by myself I cannot now be sure whether I meant to hint that the ἀπορία 'how can a subject have a plurality of attributes' is a 'bogus' problem. But I will say that if I meant such a hint, at any rate I had the manners to convey it in urbaner language, and that I also could cite great names in support of the contention, e.g., Leibniz with his criticisms of the difficulties made by Locke about the 'obscure idea' of substance).

When Mr. Hardie goes on to classify the various interpretations of the antinomies of the *Parmenides* as (a) the *eristic*, (b) the *idealist*, (c) the *transcendental* interpretations (of course classing myself among the adherents of (a)), I think I may fairly retort that the name 'eristic' is a mere dyslogistic epithet, unconsciously introduced to prejudice the reader. It would be fairer to say that I hold that the purpose of the antinomies is, as it professes to be, *gymnastic*, and that such an interpretation may be held to be recommended by the remarks which have been made earlier in the dialogue about the method of Zeno, which 'Parmenides' professes to be adopting and improving on. When we are told (1) that these arguments are a γυμνασία in the method of Zeno, and (2) that the object of Zeno was to refute the ἐνόθεσις of his antagonists by *reductio ad absurdum*, it is hard not to draw the inference which results as soon as we combine the two statements. It is true that we have to reckon with the allusions to Parmenides and the impression produced by him on the youthful Socrates in the *Theætetus* and *Sophistes*, and that Mr. Hardie is entitled to make the most of these passages, as he does. But I would suggest that (1) it is not certain that the passage

in the *Theætetus* alludes to the dialogue *Parmenides* at all, and, indeed, it can hardly be supposed to do so if the prevailing view that the *Theætetus* is the earlier should be correct; (2) the mention in the *Sophistes* of πάγκαλοι λόγοι certainly looks like a direct allusion to the *Parmenides*, but the λόγοι may, after all, be called πάγκαλοι just because Plato thinks them an admirable *reductio ad absurdum* of a thesis which he does not believe in. And I do not see that the reluctance of the 'visitor from Elea' to criticise the man who is assumed to have been his own early preceptor proves that *Plato* in another dialogue cannot have treated the personality of *Parmenides* with humour, as he freely does that of his own master, *Socrates*.

I have also to make a belated (and obvious) reply to a charge of inconsistency brought against me by Prof. E. R. Dodds and repeated by Mr. Hardie (p. 122). Why is it not open to me, or to anyone, to hold that the 'negative theology', or *via remotionis*, practised with the proper cautions, has a high value, and also that the neo-Platonic use of the method disregards the 'proper precautions', and that the *Parmenides* itself provides evidence that it is not to be interpreted on those lines? (May not a man both believe that the doctrine of the Holy Trinity is a great theological truth and also that those who have read it into the *Timæus* or the *de Cælo* have been badly astray?)

I would ask Mr. Hardie to look at the *Parmenides* again 'without prejudice'. On the face of it, it seems to offer us two interpretations of 'the One'. According to the first of them, you can make no assertion whatever about 'the One', not even that it is *one*. According to the other, you can apparently make about it (but must also deny) any statements you please. Knowing what we do of Plato, independently of this dialogue, can we possibly believe that he is offering us either interpretation as his own? Can we doubt that he is playing a game of some kind with us?

Of course it is true that an interpreter who credits Plato with a serious belief in a 'transcendent ἀρχή' has one trump card, the passage of the *Republic* about the good which is ἐπέκεινα οὐσίας. But that passage seems to stand quite alone in the dialogues, and what has to be explained, and is not explained by Mr. Hardie or by anyone else, is why if Plato had openly maintained the doctrine there and continued to hold it throughout life, he should, as Mr. Hardie represents that he does, treat it in the *Timæus* and *Laws* as something which must be kept unuttered because of the dangerous misunderstandings to which it would give rise if published. (Why, then, did he not suppress the passage in the *Republic*? Is it meant that when he wrote the *Republic* he said what he thought, 'seeing no harm', but that his eyes were afterwards opened by long experience as a teacher, though he could not recall an injudicious utterance once published?) If this is what is meant, I would still suggest that, interpret it as you will, the passage of the *Republic* implies that you can make at least two very important assertions about the ἀγαθόν.

It is *good*, and it is the *cause* of the goodness of everything else. But the 'One' of the 'first hypothesis' is declared to be something of which you can make no assertions at all. It is 'transcendent', not in the sense in which Christian theologians have held that God is transcendent (*i.e.*, that all our utterances about Him fall short of the truth), but in the sense in which Spencer's Unknowable is transcendent (*viz.*, that no significant statement at all can be made about it).

Mr. Hardie is dissatisfied with me for not finding a serious assertion of the reality of such an 'ineffable' principle in the *Epistles* (II., VI., VII.). I can bear the strictures the more easily that a benevolent French critic of my translation of the *Parmenides* actually found fault with me on the ground that I had wasted the reader's time by offering arguments to prove anything so obvious as that the neo-Platonic exegesis which uses the ambiguous expressions of these epistles to read the 'first hypostasis' into a Platonic dialogue is baseless. I would, however, remind Mr. Hardie that according to the one relevant passage of the *Epistles* which can be taken as certainly serious throughout (that in *Ep.* VII.), Plato had *never* put the doctrine to which he is there referring into writing at all. But if what is meant is rightly identified by Mr. Hardie with something which had been said both in the *Republic* and in the *Parmenides*, this assertion would be strictly and notoriously false. And I find it very difficult to take quite seriously the suggestion of page 154 that the ἄρβριος σύλλογος of *Laws* XII. is supposed to keep the doctrine of the strictly ineffable 'one' up its sleeve as the winning card in controversy with a particularly intelligent and persistent 'atheist'. Nothing is said in the account of the qualifications required of the members of the σύλλογος to suggest this. We are told only that they must be expert in astronomy, must thoroughly comprehend the theology expounded in *Laws* X. with its doctrine of ψυχὴ as the universal ἀρχὴ κινήσεως, and must be able to see the 'one in the many and the many in the one'. (They would certainly not see the 'many' in the 'one' of the 'first hypothesis' of which not even an identical proposition can be affirmed.) Nor is it clear how the practical godlessness which Plato is anxious to combat would be in any way met by any amount of insistence on the existence of this 'sort of kind of something'. I own that I still find a difficulty in making Plato's professed doctrine of the soul and of God consistent with the metaphysical scheme of the εἶδη. But I do not think the difficulty removed by simply clapping on the 'Absolute' of *Appearance and Reality* as the missing head to the trunk of the Platonic philosophy (and this is, in effect, what Mr. Hardie does). It may be disappointing to have to admit a want of final coherence in Plato's thought, but, after all, I own to finding the same kind of incoherence in other famous philosophers (notably in Spinoza's attempt to make his *Deus sive natura* who has neither intellect or will into a being who 'loves himself with an infinite intellectual love'), and I am not sure

that in principle there is more to be said in such cases than Johnson's mot, 'Depend on it, Sir, a fallible being will fail somewhere'. I might cite Mr. Hardie himself as a proof, when, *e.g.*, on page 148, he assumes that the application of the phrase *δεύτερος πλοῦς*, a 'makeshift', to the doctrine of *ιδέαι* in the *Phædo* is meant to be understood seriously, without any allowance for the habitual playful self-depreciation of 'Socrates', or at pages 151-152, where a discrepancy between the *Laws* and the *Timæus* is inferred from the very dubious assumption that the argument of *Laws* X. presupposes a time when there was a world, but a motionless world. (How can this be so, if it is true that *ψυχὴ* "moves itself" and is the cause of all other movement, and that *ψυχὴ* is 'older than' body!)

A. E. TAYLOR.

George Berkeley: A Study of his Life and Philosophy. By JOHN WILD. Cambridge (Mass.): Harvard Univ. Press, 1936. Pp. xii, 552. 6 dollars.

It is about five years ago since the author received from Harvard a grant from the Milton Fund to enable him to prepare this work for publication. An immense amount of labour has obviously been spent on it. In size and wealth of documentation it challenges comparison with Fraser's *Life and Letters of Berkeley*. But—I am sorry to have to say these things—its size is due to a quite remarkable prolixity and repetitiousness; its documentation, outside the biographical sections, will bear little inspection; and its expository sections expound not Berkeley but Wild. The biographical sections are sketchy, at a considerably remove from each other, and clearly of subordinate interest to the author, though they include a really valuable account of the Bermuda episode which only needs Mr. Luce's recent contributions,¹ here overlooked, to complete it. The philosophical sections do not, in all cases where one would expect it in a book of this size, give summaries of the contents of Berkeley's works, but, while following them chronologically, deal with them selectively to support a thesis. For example, of the *Principles* only the Introduction is closely examined, which enables Prof. Wild to say that Berkeley's best-known work "never arrives at metaphysics" (p. 68). He believes himself to have discovered a drama in the spiritual development of Berkeley, and since the presentation of this drama occupies by far the greater part, and is the *raison d'être*, of the book, I shall almost entirely confine myself to it.

¹ *Hermathena*, vol. xxiii. (1933), pp. 25 ff., and *Proc. Royal Irish Acad.*, vol. xlii (1934), sec. C, pp. 97 ff. The former prints the newly discovered petition for a college at Bermuda and the report of the law officers upon it.

The error of all preceding writers, it appears, is that they have taken the *Principles* and the *Three Dialogues* as fair specimens of Berkeley's thought. These writings, however, were immature. An adequate view must be based on the *Commonplace Book*, in which Berkeley's mind seethed with almost all the ideas which he was later to develop, and on the *Siris*, in which he reached a level of insight which, although conditioned (in a dialectical sense) by all his previous efforts, refuted and antiquated them.

Berkeley, we are informed, began life as a deist. Sincerely religious in his way, he nevertheless had a quite general and exuberant confidence in reason. For the *Principles* and the *Dialogues* tried to give a rational proof of God and immortality; his early sermons sound the rationalistic note; and the *Passive Obedience* would rest all conduct on rational laws. But his rationalism was at first held somewhat inconsistently, so long as it found reality in the given and entangled itself in the solipsism of "*esse is percipi*", and only developed its implicit Platonism in the last of the *Three Dialogues*, in which he escaped from solipsistic empiricism to apriorism by introducing the conception of archetypes existing in dependence on one another and on the infinite mind of God. Such thoroughgoing dependency meant relativism and relativism meant scepticism. Berkeley's attainment of rationalism left him in despair of reason.

This scepticism only came to a head when, in 1713, he migrated to London and saw there for the first time the shocking cultural effects of the rationalistic thinking which his writings had helped to disseminate. Disillusioned, and in an access of new religious fervour, he turned away from theory to practice, and now attacked the deists. His *Guardian* essays, the *De Motu*, even the Newport sermons, are all shot through with the sort of scepticism that takes refuge in practical certainties: the existence of God is now regarded as self-authenticating to the unspoiled mind, needing no proof, and the unreflective morality of Christian living takes the place of the rationalistic ethics of the *Passive Obedience*. But these twenty years of practicality end in a second *volte face* in the *Alciphron*. Here his scepticism of reason swings round into a scepticism of itself, which includes the negation, so far as reason is concerned, of those norms of conduct in which, in his partial scepticism, he had found a rock and consolation. The *Analyst* and the *Querist* fall within this period—hence their "query" form—but in the former his discovery of a mystery at the heart of mathematics, the stronghold of his old rationalism (for his first work was mathematical), leads him to glimpse at last the possibility of a metaphysic. The study of the sensible world had failed him and even the moral life had opened up no way to an unquestionable ultimate; but scepticism of scepticism, negation negated, involves affirmation, and Berkeley's zig-zag pilgrimage at last comes to an end in the reasonable apprehension of a religious Absolute.

There is not one Berkeley, then, but several, and of these it is

the metaphysical Berkeley of the *Siris*—usually neglected, and as much misunderstood as the earlier Berkeleys—that represents the peak, not the mere terminus, still less the expiration, of the Berkeleyan genius. The several stages, however, are closely linked; they are the phases of a lifelong effort to find an absolute whole, to press to the utmost his polemic against abstraction. From first to last Berkeley was impelled by the dialectic of a “concrete logic”, which prescribed the successive devouring of abstractions, and the phases above described do but mark advances in the extension and refinement of his application of that logic.

This is a much sobered outline of a picture which, as drawn by Prof. Wild, is overwhelming in its novelty. It has forced me wonderingly to re-read the texts on which it is based, and I am still unable to find any relation between them and it. All the usual principles of textual interpretation seem to have been suspended. In what follows I can do little more than helplessly re-affirm those principles.

Let me begin with the most general aspect of the picture—a deeply changing Berkeley. First there is the Berkeley of the *Principles*; only three years later another appears in the *Three Dialogues*, which, far from being, as is commonly supposed, “a popularized elucidation of the *Principles*”, is an attempt “to reformulate his whole system” in the light of the theory of archetypes (p. 169). In the *De Motu* and *Alciphron* a radically different Berkeley rounds upon the earlier ones, only to be in his turn repudiated and transcended by the final Berkeley of the *Siris*. What are we to make of all this? Well, Prof. Wild often emphasizes the basic importance of following Berkeley’s wish, expressed in a letter of 1730 to Johnson, “that all the things I have published on these philosophical subjects were read in the order wherein I published them”. Unfortunately, he never takes into account anything but the order of first publication; it is on this order that he rests entirely his account of Berkeley’s philosophical evolution. But Berkeley republished most of his philosophical works, some of them several times, and, to speak very mildly, this fact alone makes Prof. Wild’s theory difficult to entertain seriously. If the *Dialogues* were a re-writing of the *Principles* in the light of a radically new idea, why did he republish both side by side in the same volume in 1734? And why did he republish them anyhow in 1734, and defend them in the *Theory of Vision Vindicated* of 1733 (a work which the book under review curiously neglects), if they had been abrogated and superseded by the *Alciphron* in 1732? And if the *Siris* of 1744 rose to a final insight for which all his previous positions were gravely erroneous, why did he republish the *Alciphron*, with but slight revision, and even his much earlier *De Motu*, eight years afterwards? That Prof. Wild never shows the slightest awareness of these questions is disconcerting. His theory can only be made intelligible on the assumption that a thinker means what he says the first time he says it, but not when he repeats it after further

years of reflection. For all his emphasis on chronological order, Prof. Wild's selectiveness has resulted in an abstract order.

But perhaps a history so unhistorical, so wrong in the sense of supposing that re-affirmation is no evidence of persistence of belief, may still characterize the several beliefs felicitously. Let us suppose, then, that Berkeley reprinted nothing, and simply consider whether his works will bear the startling interpretations put upon them in this book.

Firstly, was the Berkeley of the *Principles* and *Dialogues* a deist? An author must be given some latitude in the use of terms, and when the Berkeley of those works is made a deist because he set out to find "new arguments for God and immortality by means of the logic of abstraction" (p. 112), we need only make the mental note that "scholastic" would fit just as well, or, indeed, that the description makes deists of nearly all theologians and of many philosophers. Incidentally, we are told that the root of Berkeley's deism was his "pious Arminianism" and "youthful and enthusiastic humanism" (p. 142)—the author showers "-isms" on a man who abhorred them. But when it is said that the absence of any reference to "Toland or the other Deists" in the early works proves that "his disagreement with these heterodox authors was at this time not as marked as it might have been" (p. 140); that he was at first "a typical mouthpiece of the Enlightenment" (p. 155); and that he later opposed "the rationalistic 'spirit of the age' that he had himself done so much to propagate in his earlier writings" (p. 281)¹: it is clear that we have to deal with more than the wilful meaning of a word. Berkeley is expressly assimilated to a group of men who, varying considerably in specific doctrine, form a group only because of their common negative attitude towards revealed religion. But his early writings, as much as his later ones, are punctuated with devout references, bearing all the marks of sincerity, to the revealed mysteries of the Christian faith. Prof. Wild avoids them, neither arguing their insincerity nor otherwise explaining them away.² He simply fails to see that Berkeley's confidence in the power of reason to prove God and immortality was not heterodox rationalism but an expression of the perfectly orthodox belief in the possibility of natural religion. That his confidence in reason is prominent in the *Principles* and *Dialogues* is due, as Berkeley

¹ What is the evidence that his first writings "did much" to propagate anything but a reputation for eccentricity? They were for many years met with either ridicule or neglect.

² On p. 156 he attributes to Berkeley the view that the externalities of religion are necessary only for the ignorant, and on p. 353 heightens this to an "irrational pandemonium to frighten the masses into 'obedience'". The only ground I can find for this singular characterization of a clergyman whose common sense only, never his religious integrity, has been impugned, is Berkeley's recognition that there is bound to be a difference between the religious ideas of a thinker and those of a peasant.

himself has hitherto made clear to all except his latest expositor, to their being restricted to natural religion. They do not attempt to rationalize his entire faith. Any assimilation of him to Toland is just irresponsible. As for his absence of any mention of Toland and his fellows, I see in it evidence for nothing but his delicacy: at any rate, all his writings show that, when he was dealing with his contemporaries, whom he contemned he did not name. Not one of the deists is mentioned by name even in the later writings which Prof. Wild allows to be anti-deistic.

Secondly, are the *De Motu* and the *Alciphron*, and even the Newport sermons, sceptical? That in these last he "wastes no more time on naïve Deistic 'proofs' of God" (p. 326) can only be significant to one who puts a sermon on the same evidential level as a treatise. Is a Newport congregation at a service of worship a suitable auditory for proofs "deistic" or otherwise? Even so, the very first sermon includes the heading, "Light of nature sheweth the being of a God", which is very unsceptical. The *De Motu* is called sceptical because it makes all science subordinate to practice. But this was Berkeley's avowed and emphatic view from the first to the last of his writings, and was never taken by him to involve the pragmatism of a sceptical sort that Prof. Wild reads into it. As for the *Alciphron*, we are given the extraordinary statement (p. 333, also 354) that in *Alc.* IV, 2, "Berkeley discards the Deistic 'proofs' for the existence of God as 'dry and jejune'". Having said this, Prof. Wild gives a fantastic explanation of why, notwithstanding, Berkeley thereupon proceeds to state once more the proof of God's existence he had given in the *Principles*. The proof runs that we see God; not, of course, as we see colours, but as we "see" other minds, that is, by natural inference from what we literally see. Prof. Wild's resourceful exposition of this is that "Berkeley 'proves', in other words, that we 'see' God by proving that we do not, as a matter of fact, really 'see' anything at all, but that everything 'seen' is interpretation, and hence subject to question. . . . The dogmatic certainty of the *Principles* toward 'this great truth' is now missing" (p. 334). The occasion for this interpretation is as grotesque as the interpretation itself. It happens that the statement that proofs of God are "dry and jejune" is put into the mouth of the *villain* of the piece, Alciphron himself, and even in his mouth is expressly restricted to the ontological argument and the argument from an infinite regress of causes. Prof. Wild's misreading here is a howler.¹ It is on such evidence

¹ Compare also p. 352 (again 357), where "The more doubt the more room there is for faith" (VII, 24), said to Alciphron after he has confessed complete scepticism, is taken to express Berkeley's scepticism. Again, Euphranor's words to Alciphron, "Recollect the concessions you have made, and then show me, if the arguments for a Deity be not conclusive, by what better arguments . . ." (IV, 6) become in Wild, "Berkeley, in his fourth dialogue, no longer speaks seriously of 'proving

that he finds the upshot of the *Alciphron* to be that "only the absolute atheist can see God" (p. 349), and illustrates this contention with quotations in which Berkeley does no more than he has done from the beginning, namely, except the Christian *mysteries* from the sphere of rational demonstrability. To a mind like mine, believing that at least some things have to be taken at their face-value, Berkeley's own summing up at the end of the *Alciphron* seems unequivocally non-pragmatist and non-sceptical: "The being of a God is capable of clear proof, and a proper object of reason. . . . Thinking is the great *desideratum* of the present age" (VII, 30, 31). And in the following year, in the opening paragraph of the *Theory of Vision Vindicated*, he speaks of himself as still "persuaded that the *Theory of Vision*, annexed to *The Minute Philosopher*, affords to thinking men a new and unanswerable proof of the existence and immediate operation of God". It was wickedly disingenuous of a pragmatistic sceptic to speak like that.

Lastly, was the Berkeley of the *Siris* what Prof. Wild represents him to be? I have to use this vague expression because I cannot collect with anything approaching clearness what that climax of the spiritual drama is, to communicate which is the main purpose of the book I am reviewing. When we are told that here Berkeley "at last becomes a philosopher" (p. 397) we are only given another dose of rhetoric. All this section is unintelligible to me in its obliquities of expression. "Through a knowledge (which is not knowledge) . . . reason gives up the attempt to be what it is not and becomes once more reason, or λόγος—revelation" (p. 474); and this Logos "is itself so perfect as to be imperfect" (p. 462). This sort of language does not help the reader. But, as before, I find a lack of relation between interpretation and text. The only thing that is clear to me is that Prof. Wild has taken that book of Berkeley's which needs a more cautious handling than any of the others and has simply plundered it in the interests of his thesis. If Berkeley mentions a theory of a named thinker or school without attacking it, *he*, it seems, held it. To take but two examples. In § 348 Berkeley reports that Socrates distinguished two kinds of philosophers: on page 430 the distinction is attributed by Wild to Berkeley himself. And on page 466 he quotes "the contemplation of God is the proper means to know or understand our own soul" (§ 334) as authority for holding that Berkeley had now repudiated his old view that it is by the consideration of our own minds that we know God: though when we turn to the text we find the sentence introduced by "Socrates teacheth", with no personal comment, and followed by a characteristically sudden change of reference. By such revolutionary exegesis much of what Berkeley reports as held by Plotinus is treated as held by Berkeley also.

God . . . No 'arguments for a Deity' can be 'conclusive'. The book teems with this sort of thing. In consequence, not a single quotation or reference can be taken on trust.

I have restricted myself to the author's methods of exegesis because these are the most striking features of his book and the sole grounds of his astonishing novelties. Applied to any other philosopher's works they would have equally novel results. Never before have I met in an extended work such uncontrolled interpretation, and the only explanation I can give—for it is so persistent and extreme that it needs explanation—is that the book was written up from excerpts of Berkeley's text jotted down over a long period without indicating the contexts and never re-examined. For the author is clearly an able thinker, as well as a skilful and elegant writer, and in the externals of biography is well informed and sound. Of the minor slips that none of us can escape his book contains singularly few. On the opening page Berkeley's birth is placed in 1684 instead of 1685, a mistake due to ignoring the difference made by the introduction of the new calendar in 1752. The date given by Stock of Berkeley's marriage was not 1st of April (p. 305) but 1st August, and Berkeley returned from America in 1731, not 1729 (p. 378). The *De Motu* appeared in 1721, not 1720 (p. 69), and if it was submitted to a French society at all this must have been the Académie des Sciences, not the Académie Française (pp. 258 and 263). And the *Discourse to Magistrates* was first published not in 1736, as all writers state (even Berkeley himself, or his printer, in his reprint of it in the *Miscellany*), but in 1738. Prof. Wild's dating of the Commonplace Book is less a slip than a confusion. In view of the importance he attaches to these note-books, and to the chronological ordering of Berkeley's ideas, one would expect an unambiguous assignment. On page 22 (n. 70) he maintains that note-book A (in Johnston's edition, entries 396-902) was written after the *Guardian* essays of 1713; on page 41 he quotes from this section entry 576, which is Berkeley's instruction to himself how to begin the "First Book", that is, the *Principles*, and which must therefore be earlier than 1710; and on page 48 refers to the Commonplace Book as "completed" a few years before 1709.

An appendix gives the text of three hitherto unpublished sermons of Berkeley's, apparently belonging to the Newport period, from a MS. in the British Museum (not, as Wild says, the Chapman MS., which is in Trinity College, Dublin). This is a service, for we now have all Berkeley's known sermons in print. Prof. Wild prints them "because of their importance in understanding the nature and course of Berkeley's intellectual development". The MS. contains corrections, some of which consist in striking out or amending phrases like "the light of nature", and Wild finds in these fresh evidence for his theory of a deistic Berkeley later repenting of his deism. But no one familiar with the older religious writers of Britain would regard such phrases as anything but perfectly orthodox. Why, then, the corrections? I do not know, but it is a sufficient answer to Prof. Wild that at any rate some of them were almost certainly not made by Berkeley at all. We know that the

MS. was later owned by at least two preachers, namely, his son and Dr. H. J. Rose. Mr. Luce came to the conclusion (before Wild's book was published) that the handwriting of most of the corrections was not Berkeley's, and after a recent *ad hoc* examination of the MS. I entirely agree. I am able to refer to Mr. Luce's paper, "Two Sermons by Bishop Berkeley" in the *Proc. Royal Irish Acad.*, vol. 43 (1936), sec. C., pages 271-290 for a full discussion of this question and of the sermons. This paper, as originally prepared, included the text of two of the sermons, but the appearance of Wild's book only a few days before the paper was to be read naturally necessitated their omission.

T. E. JESSOP.

Æsthetic Analysis. By D. W. PRALL. New York: Thomas Y. Crowell Company, 1936. Pp. 211. \$2.00.

Umgang mit Dichtung: Eine Einführung in das Verständnis des Dichterischen. By JOHANNES PFEIFFER. Leipzig: Felix Meiner, 1936. Pp. 76. M.2.50.

ÆSTHETIC Analysis contains a great deal of precise and analytic discussion. It is also very rich in suggestions, and I find that some of the best points are made almost in passing, so that a general account of Dr. Prall's theory does not do him complete justice. He always shows a discriminating sensibility and makes many penetrating points. A large part of the book is concerned with the function of analysis in æsthetic understanding. Dr. Prall lays tremendous stress on the importance of analysis, both in æsthetic experience itself and for our general understanding of æsthetic objects. I agree with him in this, and think that he says some very good things in support of his view.

He provides the basis for it in his second chapter (The Basis of *Æsthetic Analysis*). Here he points out and considers the intrinsic relations between sounds and between colours which generate "serial orders". Thus, for example, "By virtue of being what we call a single sound, a note has to be at a particular pitch; and to be at a particular pitch is to be at a point fixed in the single series of all the pitches that there are" (p. 46). In virtue of these relations, which Dr. Prall calls qualitative to distinguish them from spatio-temporal relations, every sound and every colour has a determinate place in a number of different serial orders. And these, he maintains, are "necessary conditions of composition in certain media" (p. 79). Sounds and colours in contrast to tastes and smells "do not merely vary; they vary systematically" (p. 48) and this accounts for the complexity of poems, pictures and music. These works of art depend on two sorts of serial order, qualitative and spatio-temporal. Dr. Prall says, however, that "to hear a note at a particular pitch is to hear a note in its relation to any and every other note" (p. 135), and here I think he is mistaken. When we hear a

certain note we only hear it in relation to another if we hear or possibly imagine the other as well.

From this basis Dr. Prall proceeds to his views on the place of analysis in æsthetic understanding, and, so far as I can see, he uses "analysis" for the various ways in which we discriminate perceptual forms. In the first place, he points out that adequate æsthetic experience involves discriminating the perceptual relations which compose works of art: "the difference between perceiving clearly and understanding distinctly is not the great difference that we are sometimes led to think it" (p. 39). But our ability to discriminate depends to a large extent on general familiarity with such relations, and so we learn to appreciate works of art by acquiring this familiarity. We may do this, for example, by listening to the differences between notes. But we can also consider these relations in a more abstract and systematic way, and this is the function of æsthetics. But however abstract our analysis may be, its final aim, in Dr. Prall's opinion, is to enrich our experience of æsthetic objects.

Dr. Prall proposes a scheme for æsthetic analysis to which I shall only refer very briefly. It includes consideration of the abstract serial orders, both qualitative and spatio-temporal, but also of the large number of basic patterns which nature provides. In outlining the scheme Dr. Prall relies to a large extent on suggestion, and I cannot always see exactly what he is proposing. But he provides two detailed examples of analysis. In chapter three (on Natural Basic Structures) he considers the western scale which is a selection out of the abstract pitch continuum and selected, he maintains, by natural conditions, for the intervals are "dictated by the heard constituents of any actually sounding note" (p. 74). And in chapter four (on Temporal Patterns) he considers the temporal pattern of English verse. Both of these investigations are very clear and interesting, and they help us to understand at least part of his scheme for analysis.

Dr. Prall supplements his emphasis on analysis by his theory of expression, which he puts forward in chapter five (on Relevance through Expression). Here he maintains that no work of art is only an abstract pattern, and that the purpose guiding artistic composition "is the artist's intention to express what he himself desires to express" (p. 141). I find this the most difficult chapter in his book, and believe it contains a good many illuminating and a good many misleading things. It is illuminating when it presents the data, and misleading when it interprets them. Thus there are some excellent points about the expressiveness of structure. For example, a funeral march "may be extremely solemn", and if we want to specify its solemnity we shall point to the tempo, loudness, timbre, and so on. Such points lead Dr. Prall to the important distinction between merely representing and presenting. When, for example, we look at a dilapidated house we may think of dilapidation, or we may see it.

But he is far less satisfactory when he discusses the difficulties which people have felt about such statements as 'this is a solemn tune'. It is, as he points out, a very important kind of statement, and many writers on æsthetics have been very worried by statements of this kind. One might put the difficulty like this: solemn is an adjective for mental states, and yet in this statement it applies to the tune. Dr. Prall attributes the difficulty to a false distinction between our feelings on the one hand, and colours, sounds, etc., on the other. Since it is difficult to see exactly what he wants to say, I shall give some quotations which show that he is trying to override the distinction between what I feel and what is sensibly presented. Thus he speaks of "the cultivated but factitious view that feeling . . . is in some mysterious way within us, and yet not 'really' or 'objectively' in our world" (p. 147), and objects to our supposing that feeling and emotion are "aspects of the spiritual and mental life, while colours and sounds, shapes and rhythms, appear as qualitative characters of material things or of physical processes" (pp. 145-146). In a number of passages he attributes feeling to perceptual objects, for example "significant æsthetic objects . . . are on the one hand single qualitative feelings"; (155) "this feeling is the given character of the picture" (p. 156). And in one passage he substitutes 'feels' for 'appears'. "Pale blue feels milder than flaming red, just as definitely and accurately as it *feels* pale blue" (p. 148, *italics mine*).

Now it strikes the reader at once that the "factitious" view to which Dr. Prall objects is perfectly true, and that his use of "feeling" is extremely queer. I believe that Dr. Prall's statements should be interpreted as a recommendation to alter our language, though he does not himself present them as such. And the points which he brings forward in support of them should be interpreted as points in favour of this recommendation. Thus, on the one hand, he draws attention to the *similarities* between our feelings and sounds, colours, etc., while, on the other hand, he raises the spectre of metaphysical dualism. I admit that our ordinary language loses sight of these similarities, and that it suggests the metaphysical theories—to philosophers. But Dr. Prall very much exaggerates the evil. The theories are not, as he suggests, part of our ordinary beliefs, and there is, of course, a real distinction between what I feel and what appears. Our language is not as arbitrary as he would have us think. And finally, I do not believe that the distinction between what I feel and what appears need prevent us from overcoming our perplexity about solemn tunes.

In conclusion, I should like to make some remarks about chapter one, where Dr. Prall discusses the subject matter of æsthetics. Here he maintains that æsthetics is a science, on a level with the natural sciences, but with a special subject matter, namely, "qualitative presentation". It is the scientific study of sense qualities and their sensible relations. I feel dissatisfied with this for two

reasons. It is of course clear that æsthetics is concerned with sensible appearances, but it also discusses a number of subjects which do not come under this description. One must remember that Dr. Prall is using 'qualitative presentation' to include emotional response. But æsthetics, in the "established meaning" which Dr. Prall is considering, also discusses for example the nature of value predicates and of meaning in poetry. I do not agree with Dr. Prall that it is "not primarily concerned with value judgments" (p. 179). And there is also another point. I do not deny that there is or may be a science of æsthetics. But æsthetics is also a branch of philosophy, and a good deal of what Dr. Prall discusses comes under this head. And I believe that philosophy is radically distinct from science.

I would like finally to express my sympathy with Dr. Prall's insistence on the need for "precise specifications" about works of art in distinction from vague expressions of approval or disapproval. This insistence influences most of his book, and in my opinion is the most valuable aspect of it.

Umgang mit Dichtung is a much shorter book. It does not carry any discussion very far, but is excellent within its limits. It is a useful introduction to the problems of poetic language, and for the most part is successful in avoiding vague generalisation. A great merit lies in the aptness and number of its illustrations. Dr. Pfeiffer nearly always quotes several poems to supplement what he says, and accordingly communicates his views by example as well as statement. He first discusses the nature of poetic language, and then the criteria for judging it. It may be as well to point out that he uses 'poetic' so that the word may apply to prose, a point which is confirmed by his ending the book with a prose example. Language is poetic when it satisfies certain conditions, which do not include rhyme and metre.

Dr. Pfeiffer opens his discussion by contrasting poetic language with sentences which state an idea or fact. Poetry presents its subject matter in a special way, and we want to ask, what is the subject matter? and, how is it presented? The subject matter, so Dr. Pfeiffer answers, is "something felt and experienced", a mood (*Stimmung*) or, in other words, an emotional attitude, for he is using 'mood' in a very wide sense. And how is this attitude presented? Here Dr. Pfeiffer uses a variety of familiar expressions, it is "transformed into verbal form" it is "*leibhaftig da*", and so on. It must be admitted that such expressions have been used again and again, and are far from precise. But they do their work quite well, even though we may want to get behind them. Dr. Pfeiffer also makes the distinctly helpful remark that poetry has close affinity with expression through tone of voice and gesture. His main conclusion is to stress the importance of verbal form. It is kernel as well as shell, and the meaning of a poem can only be given in its own words. Accordingly, he takes untranslatability to

be the final criterion of poetic language, meaning that a poem cannot be translated without loss.

It is now time to say that Dr. Pfeiffer does *not* rest content with the familiar expressions mentioned above, and that he does go beyond them. He points out some of the facts about verbal form on which its expressiveness depends. He maintains that it depends both on its sound and on its meaning. He believes that both these aspects are important, and this is one of the merits of his treatment. Under the head of sound he discusses rhythm and melody, under the head of meaning he discusses imagery. At first he maintains that images are essential to poetic meaning, but admits finally that some poems do not use them. And then he falls back on their expressiveness and untranslatability. In other words, he gives up trying to explain. His further discussion about poetic imagery is interesting, but he over-generalises. For example, he claims that implicit is superior to explicit comparison. The image should represent something which is not itself mentioned. In other words, he takes the simile to be an inferior form of imagery. But one can hardly deny that some excellent poetry, *e.g.*, that of Donne, employs the simile.

Dr. Pfeiffer then proceeds to discuss the criteria for judging poetry, and first he considers three pairs of criteria which refer to success or failure in expressiveness. A poem may be genuine or not (*echt* or *unecht*), and it fails to be genuine if, for example, there is a clash between its theme and its tone. A poem may be original or derived (*ursprünglich* or *nicht-ursprünglich*), and thirdly, it may transform its subject into words or merely speak about it (*gestaltet* or *geredet*). The third distinction seems to me to be more general than the others. We can say: This poem fails to express its subject *because* it is not genuine. But when we describe a poem as *geredet*, are we saying more than that it fails to express its subject? If we are, I have not been able to see what more.

These, however, are not, in Dr. Pfeiffer's opinion, the only criteria of poetry. Under the heading, "What do we Get out of Poetry?" he maintains, and I think rightly, that the value of poetry also depends on what is expressed. There are, he says, both æsthetic and ultra-æsthetic criteria. Provided that two poems both achieve successful expression their relative value depends on what they express. Finally, he says that although poetry does not state but express, yet it reveals the unconscious depths of our being. But the revelation is only given to us if we concentrate on poetic form.

HELEN KNIGHT.

VII.—NEW BOOKS.

Die sogenannten Definitionen durch Abstraktion. Eine Theorie der Definitionen durch Bildung von Gleichheitsverwandtschaften (Forschungen zur Logistik und zur Grundlegung der Exakten Wissenschaften, Heft 3). By HEINRICH SCHOLZ and HERMANN SCHWEITZER. Leipzig: F. Meiner, 1935. Pp. 106. M. 4.50.

PROF. SCHOLZ, who is editing the series of logistical studies to which this work belongs, has the rare distinction of being both an accomplished logician and an erudite philosophical scholar. It is in the latter rôle that he appears in this work; for his part in it is limited to a critical exposition of the various forms of "definition by abstraction" that are to be found in the history of logic. Having shown that none of them can be regarded as wholly satisfactory from the standpoint of an exact logic, he leaves it to Dr. Schweitzer to work out in logistical terms a form of definition which shall serve the same purpose as they were designed to serve without being open to the same objections. The symbolism used agrees in the main with that of *Principia Mathematica*. It differs in having $\dot{x}\phi x$ as a symbol for 'there is an x , such that ϕx ' and $\bar{x}\phi x$ for 'for all x , ϕx ' and also in its signs of conjunction, implication and equivalence which are, \wedge , \rightarrow and \leftrightarrow , respectively.

It is widely believed that the procedure of forming concepts by abstraction, which plays so large a part in pre-logistical, and particularly in mediæval, logic, goes back to Aristotle. To this Prof. Scholz raises objections which, in my opinion, are decisive. He does not deny that Aristotle made use of a process of abstraction; but he shows that he regarded it not, as did the mediæval logicians, as a method of obtaining universals, but as a method of arriving at particulars; for it is clear from the references given by Prof. Scholz that Aristotle understood by τὰ μαθηματικά not mathematical concepts but things. A further point of difference is that whereas for the abstraction of universals a multiplicity of similar empirical objects is required, Aristotle appears to allow the possibility of 'abstracting' his ideal things from a single empirical object. Indeed, all that the two processes appear to have in common is that they both set out from the consideration of empirical objects. The Thomist proposition "nihil esse in intellectu, quod non prius fuerit in sensu" which is an essential part of the post-Aristotelian doctrine of abstraction, reproduces Aristotle's "οὐδέποτε νοεῖ ἀνευ φαντάσματος ἢ ψυχῆ", though admittedly in a strengthened form.

Prof. Scholz agrees with the Port-Royal logicians in condemning this empiricist principle. Because of it, he thinks, the 'classical' process of abstraction, as exemplified for instance in the writings of Boethius, Abelard, John of Salisbury, Locke and Hume cannot be regarded as a logical process at all, but only as psychological. For he maintains that the only processes which should occur in a 'well thought-out' logic are processes of

axiomatisation, definition, deduction and the transition from propositions to propositional functions or its converse; and the abstraction of universal concepts from sense-data does not fall under any of these heads. Without wishing to quarrel with Prof. Scholz's conception of formal logic, I should hesitate to condemn the classical doctrine of abstraction, as he appears to do, merely because it has been wrongly labelled. He has, however, a more serious criticism to make of it which is that it is too narrow in its application. For it yields only qualities, that is the objects designated by one-termed predicates, and furthermore only such qualities as actually have empirical exemplification. He contrasts this unfavourably with a logistical technique of abstraction, based on the ideas of Frege, which applies to any concept (the designation of any $1 \dots n$ -termed predicate) whether it is empirically fulfilled or not and is, as we shall see, a purely logical process in his own strict sense.

He passes next to an exposition and criticism of the processes of abstraction by which Cantor thought to obtain the cardinal and ordinal numbers. He shows that they presuppose the validity of the following principle, not explicitly stated by Cantor. "In the case of any two-termed relation G , which is symmetrical, transitive and reflexive for all possible arguments, it is possible by means of a process of abstraction carried out upon the elements of the field of G to obtain one and only one function f , such that (1) $f'x$ designates the object which any x and y , for which xGy , have in common; and (2) $f'x = f'y \iff \langle x, y \rangle \in G$." To this Scholz justifiably objects that it is not certain that any such function is obtainable in every case, and that there are some cases in which it is certain that more than one such function is obtainable. Thus, we cannot say, for example, that the cardinal number of α is what α and β have in common when α is similar to β , in Cantor's special sense of similarity; for the equation $f'x = f'y \iff \langle x, y \rangle \in S$ will be equally well satisfied if we take say the square of the cardinal number, instead of the cardinal number itself, as our value of f . He raises also the further objection that the injunction which Cantor gives to abstract from the order of the elements cannot be observed in the case of unit-aggregates, and that in the case of null-aggregates his injunction to abstract from the quality of the elements cannot be observed either.

The objection that there are cases in which more than one function will meet the required conditions applies equally to the form of definition by abstraction advocated by Peano, which is again an equation of the kind; $f'x = f'y = \text{d.f. } xGy$. Scholz also follows Frege in arguing that it is not satisfactory merely to define " $f'x = f'y$ " without defining " $f'x$ ". We require, for example, not merely a definition of "having the same cardinal number" but also a definition of cardinal number itself. And this Peano's method does not give.

The rest of Prof. Scholz's historical essay consists of a brief exposition, without criticism, of Frege's interpretation of number and Russell's "principle of abstraction", a criticism of Weyl and Dedekind as followers of Cantor, a sharp and excessively summary attack on the logic of Burali-Forti, and an account of the attitude adopted towards Peano's method of definition by abstraction by three of his followers. There is nothing here that calls for any special comment. We may turn, therefore, to consider the contribution of Dr. Schweitzer to the theory of 'definition by abstraction' which, together with an appendix on Frege's method of introducing names for the extension of propositional functions, makes up the remainder of the book.

It is characteristic of terms which are said to share a common property that they are related by a symmetrical, transitive relation. The definition of 'symmetrical' and 'transitive' as applied to two-termed relations is given by Dr. Schweitzer as follows: $R \epsilon sym =_{Df} (\dot{x} \dot{y} xRy) \wedge (xRy \leftrightarrow yRx)$

and $R \epsilon trans =_{Df} (\dot{x} \dot{y} \dot{z} xRy \wedge yRz) \wedge (xRy \wedge yRz \rightarrow xRz)$. The advantage of these over the usual definitions is that they rule out the paradox of the Null-relation's being both symmetrical and asymmetrical, transitive and intransitive. As thus defined, every symmetrical, transitive relation is reflexive, though not necessarily totally reflexive. A reflexive relation is defined by Dr. Schweitzer as one which is not null, and is such that every member of its field has it to itself; a totally reflexive relation as one which is not null, and is such that every element of which it can be significantly asserted has it to itself. Relations which are symmetrical, transitive and reflexive he calls "Gleichheiten", those which are also totally-reflexive "Totalgleichheiten".

He proceeds then to show that the elements of the field of a "Gleichheit" R can be ordered in one and only one way into classes, such that: (1) the classes contain only elements of the field of R (not elements which lie outside the field of R but within its range of significance); (2) every element of the field belongs to at least one class; (3) no element belongs to more than one class; (4) any two elements belonging to the same class stand to one another in the relation R; (5) no element stands in the relation R to any element outside its own class. Every such class he calls a "Gleichheitskreis" with respect to R. Having made " $\alpha \neq 1$ " part of the definition of any "Gleichheitskreis" α , he shows that the class of "Gleichheitskreise" with respect to a "Gleichheit" R is identical with the domain of R, excluding the null-class.

It is this construction of "Gleichheitskreise" that Dr. Schweitzer puts forward as a logically satisfactory substitute for the process of definition by abstraction. He is able, as Peano was not, to define $f'x$ uniquely; namely, as the "Gleichheitskreis" with respect to R in which x lies (in symbols: $f'x =_{Df} \alpha(\alpha GkR \wedge x \epsilon \alpha)$). His claim that this definition accomplishes what Peano's was intended to accomplish is supported by the fact that he is able in conformity with his definition to prove the proposition " $f'x = f'x \leftrightarrow xGy$ ". He gives several other examples to illustrate the fertility of his method both with respect to two-termed relations and also with respect to four-termed relations, such as Hilbert's relation of congruence between points. It is only with reference to a two-termed "Gleichheit" that we can speak of a "Gleichheitskreis". In the case of four-termed "Gleichheiten" what corresponds to the class of "Gleichheitskreise" is a class of 2-termed relations called by Dr. Schweitzer "Gleichheitsverwandtschaften". Their definition and application is analogous to that of the "Gleichheitskreise". Thus, every pair which belongs to the field of the four-termed relation of congruence is shown to belong to the field of one and only one "Kongruenzverwandtschaft". The length of AB may, accordingly, be defined as that "Kongruenzverwandtschaft" in which A and B stand. There is no need for me to quote further examples. I hope that I have said enough to make the nature of the method clear. It can of course be generalised so as to apply to $2n$ -termed "Gleichheiten", where $n > 2$, as Dr. Schweitzer shows.

Does this method of definition really accomplish what the authors claim for it? That it has a logical precision and elegance which the 'classical'

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theories lacked may be conceded without further ado. There remains still the question: Does it achieve the same end as the 'classical' processes of definition by abstraction were intended to achieve? In my opinion, it does not. For the concepts which these definitions by abstraction were designed to obtain were universal-concepts; whereas Dr. Schweitzer's "Gleichheitskreise" are particulars. The former, being universals, could significantly be predicated of terms other than those from which they were supposed to be abstracted, and the propositions making such predications were synthetic. This is not true of the "Gleichheitskreise", as can be easily seen by taking a simple example. Consider the symmetrical, transitive, two-termed relation of having the same colour. With respect to this relation it is possible to form a number of mutually exclusive classes which satisfy the conditions laid down by Dr. Schweitzer for his "Gleichheitskreise". But the only way in which one such class can be distinguished from another is by an enumeration of its members. It follows that every proposition which ascribes a particular element to a particular class (for example, any proposition of the form " x is blue") must be either tautologous or self-contradictory. It must say, on this interpretation, either that x belongs to the class to which only $x, y \dots$ belong or else that x belongs to the class to which only $y, z \dots$ (and therefore not x) belong. This is an objection which, in my opinion, is fatal to any attempt to replace universals by classes, at any rate so long as one interprets the notion of a class extensionally, as Dr. Schweitzer does. It is noteworthy that he refers to Carnap's *Logische Aufbau der Welt* as an illustration of the utility of a method of definition which he recognises as being similar to his own. But, as Popper and others have shown, it is impossible to constitute 'universals' in the *Aufbau*; and this is its essential defect.

Although, for this reason, I am unable to regard his attempt to find a substitute for the process of 'definition by abstraction' as having altogether succeeded, I should like to end my review by expressing my admiration for the mastery which Dr. Schweitzer displays of logistical technique. I believe that this is characteristic of all the leading members of the "Münster group". It is a pity that in England their work is so little known.

A. J. AYER.

Aristotle's Criticism of Presocratic Philosophy. By H. CHERMISS. Baltimore, Johns Hopkins Press; London, H. Milford, 1935. Pp. xiv + 418. 18s.

MR. CHERMISS has produced a very learned and minutely documented work which will certainly be of the greatest value as a standard book of reference to all students both of Aristotle and of the Presocratics, but which is extremely difficult to review properly in a necessarily brief notice. The difficulty is that there is a certain doubleness of aim running through the volume. Ostensibly the author's main purpose is to argue (1) that Aristotle, being out of sympathy with the whole drift of early Ionian philosophy, has constantly misunderstood the authors whom he criticises, and (2) that the doxographical tradition about them, which goes back to Theophrastus, may be presumed to presuppose the Aristotelian misconceptions, and therefore be largely discredited as unhistorical. This being so, the problem is to recover the real sense of Ionian philosophemes which have been misrepresented by Aristotle, and to interpret the Ionian systems

with genuine historical insight. The argument, however, resting as it does on the consideration that Aristotle's discussions of his precursors are regularly intended not so much as a contribution to historical knowledge, but rather as dialectical expositions of *ἀπορία* to which he believes himself to hold the only key, requires to be made out by a long and minute account of Aristotle's own theories of nature and the extent of our 'natural knowledge'. For, as Mr. Chermiss truly says, we cannot estimate the worth of an Aristotelian criticism of a precursor apart from consideration of the context in which the criticism is made, and the purpose for which it is introduced. Unfortunately the inevitable consequence is that all but the last two of the seven chapters of which the book consists are given up to a close and minute account of Aristotle's own views on methodology in the science of nature, and the detailed contents of the natural sciences from cosmology right down to embryology. Throughout three-quarters of its compass the work thus becomes rather a contribution to the study of Aristotle than a discussion of his treatment of the precursors whom he is so fond of setting right. In fact, only in the last 70 pages do we really come face to face with the proposed subject of the whole. Mr. Chermiss gives sound reasons for his proceeding (though I doubt whether they quite justify, for example, the amount of space devoted to such relatively minor problems of embryology as those discussed on pp. 276-288), but a mass of over 330 pages devoted to an exposition of things which the reader will know already from his own study of Aristotle or from standard works of reference like the latest edition of 'Zeller', makes heavy reading, and a reviewer may be pardoned if he dismisses most of it by saying that, like Mr. Wopsle's 'reading' of the character of Hamlet, it is 'massive and concrete', and concerns himself more specially with the relatively small part of the whole which actually deals with the topic announced on the title-page. Of the exposition which fills so much of the book I must content myself with saying that, so far as I am competent to judge, it is throughout scholarly and accurate (though I cannot profess to have verified more than a comparatively few of the author's hundreds of references to Aristotelian texts which he only very rarely deigns to quote).

No exception can be taken to the introductory statement of the reasons which make an investigation of this kind indispensable, except that Mr. Chermiss is perhaps a little ungenerous in assuming that other students have not been as much alive as he is himself to the considerations that Aristotle commonly cites precursors only to raise some *ἀπορία*, and that the *ἀπορία* said in different places to be suggested by a particular doctrine of a precursor are not always the same. I am certain that Burnet, for example (whom Mr. Chermiss seems particularly anxious to put in the wrong as often as he can), was perfectly alive to all this. Nor was he, I think, blind to the possibility that doxographic statements going back to Theophrastus may sometimes originate in an Aristotelian misconception or misrepresentation. I think, however, that he would have doubted, and I should say have rightly doubted, whether this antecedent possibility is sufficient reason for discounting explicit statements of Theophrastus quite as readily as Mr. Chermiss seems prepared to do. (The remains of his *Metaphysics* might suggest that he was far from a slavish devotee to his old master's *ipse dixit*.) Still, Mr. Chermiss may excusably feel that it is precisely the scholars of the generation immediately before his own whose *i*'s and *t*'s it is particularly imperative for him to dot and cross.

Of course the task he has set himself when he does "get down to brass tacks", as they say, in his final chapter, is difficult, if not desperate. We

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have so little to go on in our interpretation of the Presocratics, beyond Aristotle's own criticisms, the isolated passages quoted in illustration of these criticisms by the Aristotelian commentators, and the general Theophrastean doxographical tradition, that when Aristotle is discredited as wanting in genuine interest in and sense for history, the doxographical tradition as infected by Aristotelian influence, and the 'fragments' as not adequately intelligible in detachment from their lost context, it may well be asked whether, the hypothesis being granted, we have any material left from which we can reconstruct the real thought of the Presocratics with any confidence. Should not the conclusion be drawn that the period before Socrates belongs to 'prehistory', where we are never on firm ground? Mr. Cherniss does not quite take this view; he has much to say which demands serious consideration against various 'reconstructions' by other recent writers, particularly against the extreme scepticism of E. Frank about the Pythagorean tradition. But I am myself doubtful as to the worth of the principle of which he makes great use in discussing the accounts given by others of the thought of the Milesians, Heraclitus, Anaxagoras, Empedocles, the Atomists. Their *dicta*, it is assumed, will be found to be coherent and intelligible when it is remembered that they were consistent believers in a 'mechanical' theory of nature; they only appear to contradict themselves or to be incoherent to Aristotle because of the rooted 'finalism' of his own view of natural process. I do not know just what Mr. Cherniss understands by a thorough-going mechanical theory of nature, and indeed, I am not sure whether the phrase has any very definite meaning. (Dr. Whitehead has more than once hinted that it has not, and I must say I suspect he is right.) But at least I should say that to be wholly 'mechanistic' a philosophy must at least reduce all qualitative differences in the material world to differences of a more and a less of something, and must do this consciously (as Descartes or Malebranche did for the world of bodies). Now I can understand any one who says that the philosophy of Anaximenes or that of the Atomists is 'mechanistic' in this sense, and that the same thing is even more true of the *Naturphilosophie* of Plato's *Timaeus*. But it does not seem to me that there is any evidence that this reduction of quality to quantity played any part in the thought of Anaximander, Heraclitus, Anaxagoras or Empedocles; in fact, the evidence we have seems to me to suggest that with all of them qualitative differences were treated as something ultimate, as they are by Aristotle himself. None of them, so far as I can see, had ever even dimly proposed to himself the kind of ideal of natural knowledge which Descartes had before him when he sat down to compose his *Monde* or his *Principia*.

Similarly I feel doubtful when, after pointing out, as others have done before him, that the Pythagoreans are sometimes represented by Aristotle as saying that things *are* numbers, sometimes as saying that 'the elements of things are the elements of numbers', Mr. Cherniss argues that the same school cannot have made two such conflicting assertions, and that the second of the two *must* therefore be a polemical misrepresentation or misunderstanding due to Aristotle himself. If Cartesianism were only known to us from such scanty materials as Pythagoreanism, it would be easy similarly to show curious discrepancies in our reports. Thus the reports might preserve both Descartes' declaration that mind is demonstrably immaterial and the thesis of Regius that, though known independently of matter, it may be *corpus quoddam*, or both sentences from Descartes about the 'innate' idea of God and others from Malebranche which deny

that God can be known by means of an 'idea'. And yet a historian who inferred from this that the propositions of Regius and Malebranche have only been ascribed to them by a polemical misrepresentation would be mistaken. In our almost total ignorance of the history of fifth-century Pythagoreanism, we are really not in a position to say what degrees of divergence of view were possible within the society. I would at once admit that Mr. Chermis's theory of what was and what was not the genuine teaching of the school is one which should be seriously considered with an open mind, but it is surely going too far when he treats it as demonstrated truth and convincing evidence of Aristotle's inherent untrustworthiness.

It is a small point, by comparison, but worth noting, that in the attempt to set aside the evidence of Aristotle and Theophrastus as to Plato's belief in the earth's motion (p. 393), Frank's very pertinent reference to *Critias* 121c is curtly dismissed with the remark that 'little can be made of a broken sentence from a myth'. Little, no doubt, but I should say enough. For the sentence speaks of Zeus as assembling the gods in his most honourable dwelling-place which is at the centre of the universe, and overlooks *all things which have part in becoming* (the italicised words are naturally not included in Mr. Chermis's quotation of the passage). Now Zeus is so notoriously the god of the sky that no Greek myth is likely to put him underground; it is a *Zeûs ἄλλος* who is said by the poet to reign there; and the centre of the earth is not likely to have struck Plato as a specially suitable observation-post from which to look down on (*καθ'ορᾶν*) everything that goes on.

A. E. TAYLOR.

Der Aufbau des Organismus. By KURT GOLDSTEIN. The Hague, M. Nijhoff, 1934. Pp. xii + 363. 8 Gulden.

DR. GOLDSTEIN is one of those physicians who were fortunate enough to be in a position to profit by the ravages of war, and he has already published important information on the subject of brain lesions. Just as Head always gives the reader the impression of being more interested in what his patients can do rather than in giving us a list of their deficiencies, so Goldstein, and his former collaborator Gelb, impress one as being less interested in establishing the nature and extent of a lesion than in watching, with an understanding eye, the attempts made by the organism to cope with its disabilities.

It is for this reason that Goldstein's opinion is of value on the question of the best way to envisage the nature of living organisms: he has had great experience, and he has a 'sympathetic' vision.

As one might imagine, his book is an attack on the 'mechanical' interpretative frame-work, according to which a functioning organism is made up of a number of inter-acting reflexes. Indeed, he calls into question the value of the concept of reflex for the understanding of living organisms altogether.

The general lines of his criticism are fairly familiar. The reflexes which can be elicited and named turn out to work only under special conditions. Sometimes the same stimulus in the same place will call forth different responses; the response may vary with the distribution of attention, it may vary with the position of the other parts of the body, or according to the stimulations which have gone before. Even the

pupillar reflex, which seems constant enough, behaves differently when we are looking at something, from the way in which it behaves when it is artificially stimulated.

In fact, a given reflex can be elicited except when it cannot be elicited, and in order to account for those cases in which the reflex fails to work, the 'mechanical' theory has to invent inhibitive relationships and control centres. These, it is complained, merely botch up an unsatisfactory theory, and give us no clue to the nature of the organism as a whole.

When we turn to that vast collection of material which is concerned with the extirpation and transplantation of nerve substance, the non-specificity of substance is so astonishing that mechanical interpretation becomes impossible.

Goldstein insists that if we are to get an adequate frame-work for the understanding, not only of the organism in a state of healthy nature, but of the organism under laboratory conditions, we have to take the organism as our unit. We have to realise that the reflexes, when they are elicited, and when they betray the constancy which makes us give them a name, are responses of the organism under special conditions. These conditions involve the artificial preservation of a state of equilibrium in the rest of the organism, on the background of which the reflex-response is executed. We remember the precautions that Pavlov had to take to keep the attention of the dog concentrated on the food-bell combination; we remember that when our knee-jerk is tested we have to detach the rest of ourselves from our knees, and leave them to the experimenter to tap. No wonder, then, that under normal conditions, when this artificial isolation of a part of the organism is absent, the reflexes cannot be elicited according to the rules.

The organism is conceived of as an individual totality, which has certain tasks to perform; it has to keep its end up in the environment, but there is something more to it than that. It must not be conceived of as a system which gets jolted out of equilibrium and seeks to return to rest; each individual organism has a nature to express, so that the mean round which its behaviour hovers is not stillness but an optimal condition of tension peculiar to itself. It is this which makes it possible for us to overcome difficulties, and to enjoy doing so, and because of this we have developed a technique for dealing with possibility. Here it is that the healthy man differs from the sick, and particularly from the man suffering from brain lesions: the patient suffering from a brain lesion cannot imagine alternative ways of getting round difficulties. Goldstein mentions the case of a patient, whose arm was loosely held while an irritating stimulus was applied; he would normally have used the arm that was held to scratch the irritated spot, but he could not free it. He struggled wildly, but did not attempt to get at the spot with the other arm. When, however, his arm was so tightly held that movement was absolutely impossible, he immediately scratched the spot with the free hand.

The injured organism is, therefore, able to do something to keep its end up, but centralisation has been undermined; there may be activities which it can perform, but the organism has not got them under central control any longer, and special conditions are required for their elicitation.

Indeed, the capacity of the organism to develop alternative modes of satisfying its needs is extraordinary. We all know of cases in which people who have lost their right arms are able to write with their left hands without the difficulty which ought to be present if the mechanical theory were true, and we have read of the astonishing capacity of amputated insects,

frogs and other animals to get about with the limbs which have been left at their disposal, without having to be taught what to do.

Goldstein refers to several cases of his own. A patient suffering from hemianopia developed a 'pseudo-fovea' in the sound halves of his eyes; another whose vision was affected was able to read by tracing the script with his finger without realising what he was doing.

An important factor in encouraging the spontaneous formation of alternative techniques is the pressure on the organism. Head has told us that aphasiacs may not be able to say words after a doctor, but that when they 'really want' to use a certain word they may very well be able to utter it. Nystagmus is reduced when a patient is told to look at something. Goldstein mentions a patient whose left arm was stretched away to the left when he turned his head to the right, and when in this position he could not point with his left hand to a person standing on his right, if he were asked to do so. When, however, he was standing in a normal position and wanted to point to someone on his right, he could do so, and also turned his head to the right at the same time.

The whole-wise working of the organism is further illustrated by the 'privileged postures' which we take up as a convenient background to various performances. If we want to point to something, there is a characteristic stance which we adopt, and which comes to us as the 'natural' position, while other positions feel 'unnatural' and therefore uncomfortable. It is true that we are sufficiently centralised to be able to point from various 'unnatural' positions, but the fact that we always take up certain positions for the performance of certain actions indicates that there are certain dispositions of energy which are more characteristic for the nature of the organism than others.

Goldstein found that his patients adopted postural constants which differed from those of normal persons, and that when they were forced to adopt the normal stance they could not do the things which were possible for them with their new 'natural' postural background. In the case of one patient, her discrimination of colours was less efficient when she was in a normal posture, than when she was in a posture 'natural' to her pathological condition.

If we accept Goldstein's arguments, if we agree that we cannot build up the organism out of reflexes, but rather have to understand reflexes themselves as limiting cases of isolated response on the part of an artificially or pathologically de-centralised portion of a whole, which under normal conditions is centralised, then we shall have to admit that there is a limit to our powers of generalisation. Every organism is an individual which has its own characteristic equilibrium, and therefore its own 'tasks' to perform, and we only know that it will seek the best possible methods of preserving that equilibrium. We know that when the organism is wounded or otherwise injured it will make some attempt to reinstate a balance at a lower level of vitality, but what it will do will depend on the nature of the organism itself, and that can only be discovered by particular inspection.

This is a position which alarms a great many people, and for that reason Goldstein's thesis, carefully argued and supported by his own and other people's evidence, is of value.

W. J. H. SPROTT.

The Scientist in Action, A Scientific Study of his Methods. By WILLIAM H. GEORGE. London: Williams & Norgate, 1936. Pp. 355. 10s. 6d.

THIS book should prove stimulating to scientists, methodologists, and philosophers of all kinds, speculative, critical, or logico-positivist. It is written in a refreshingly vigorous style, and is attractively got up. The theme is original and is handled with patience and care. The work is of sufficient importance to be essential to a methodologist. Students, too, should find it very useful. The only criticism that might be suggested is that the book might have been compressed with profit.

To enter into the soundness or otherwise of the author's attitude to methodology would be too vast a question for a brief notice. It seems best to give a fairly full account of the subject; for, though the theme is simple, it is difficult to convey in a word or two how original the theme and its treatment are. Dr. George has given a scrupulously careful description of the scientist's behaviour. He divides it into three parts: (1) the scientific outlook, (2) getting scientific facts, and (3) the arrangement of scientific facts.

The general character of the thesis is that questions of truth in any absolute sense do not produce any response in the behaviour of the scientist. Dr. George is "uncompromisingly non-philosophical. Nothing could be more unrepresentative of the viewpoint of the vast majority of research workers than an analysis of scientific research in terms of philosophy." He takes it as basic that "scientific research is a form of human action". This form of action leads to two results, discovery of *facts* and their arrangement in a *pattern*. Facts observed by the scientist are *observed coincidences*, they are pieces of impersonal knowledge, yet they are all a *biological product*. The fitting of facts together to form a pattern—*or* scientific hypothesis—is followed by the discovery of further facts which fit the pattern. "The technique of scientific research is the only device known to man at the present time by which a fact may be established. . . . Research workers then like a pattern to be complete, *i.e.*, complete as regards being a pattern of facts already discovered, but a pattern with gaps into which hitherto undiscovered facts may be fitted."

With regard to Absolute Truth or the Real, it is interesting to find a scientist stating views that coincide with those of the logical positivists. According to Dr. George's account, statements in research papers usually begin: "'Let it be supposed that . . .'" or some such form. In all the examples which I have examined these statements never say anything about 'real' or 'unreal'. If one added to the postulates an additional one in the form 'Let it be supposed that the electron is real' (or unreal according to philosophic taste) then it will be found that no additional equation can be written and not the slightest change has to be made in the mathematical treatment already given without this additional postulate. If this purely philosophical idea of 'real' or 'unreal' is ignored in doing the piece of research it is difficult to see how it can logically, within the scope of science, be attached to the products of the research when all the work is done" (35, *cf.* also p. 272). Again when asked how he knows that in a coincidence observation two objects really coincide, he replies: "Since the patternist treats facts (as technically defined) as basic, and does not use the idea of truth (in any philosophical sense of the word) he would at once reply that he never knows whether two objects 'really' coincide. He has taken facts (coincidence observations) as basic, and must therefore define 'really' . . . in terms of coincidence judgments

made by human observers" (97). Having discarded the idea of absolute truth as irrelevant to science, the author replaces the law of Uniformity of Nature by the policy of re-observation: In the same circumstances the same facts can be observed.

This angle of approach leads up to a discussion of what Dr. George calls the "Should-Ought Mechanism". Whenever the words "should" or "ought" are used in an absolute sense, such usage is itself but an instance of reaction to environment. Thus a small boy may say that his toy engine *ought* to have spring buffers; or a weighing machine may tell us that a man of forty, of height 5 feet 10 inches, *ought* to weigh 152 lb. In scientific action such ideas have no place, and the words in question do not occur in scientific papers. Closely allied is the conception of Assessment of Value. A characteristic feature of statements assigning value is that they cannot be technically verified. Assessment of value, like research technique, is a device for ordering facts, but it is never a part of research technique. "Human beings must be accepted as they are made and all men are so made that, long before they are old enough to do research, they have become fully accustomed to apply the Should-Ought Mechanism and assessment of value. They continue to do so throughout life whether or no they are ever scientists. Therefore these two cornerstones of research consist of only a temporary abandonment of two devices which are normally in frequent use. That the abandonment is only temporary is shown by the fact that when a piece of research work is completed, research workers apply assessment of value to it. The abandonment is only in the actual practice of research." (70).

For reasons of space I will pass over the second part of the book which contains less that is striking and original. In Part III. the arranging of scientific facts is considered as a kind of pattern-making. A pattern consists of elements and relations. Scientific theories are patterns into which facts may be fitted. Order, a thing that characterises scientific theories, becomes a pattern-property: it is a property of the patterns that scientists make for facts. Thus patterns are essentially man made. Dr. George does not, therefore, talk of scientific theories as being true, for this might imply a metaphysic of Absolute Truth. But considered as patterns, some will embrace more facts than others, and it is a *fact* that scientists show a liking for those patterns that cover the most facts. No pattern is true, or truer than another, though one pattern may be preferred to others. This preference may depend upon the purpose for which the pattern is used. Thus the assessment of value is a patterning device, and one used by everybody including scientists; but it is no use to a scientist engaged in a scientific activity, and at such moments he prefers some other patterning-device. This attitude reminds one of Prof. Carnap's view of alternative logics: we cannot say that a logic is true or that it is truer than another, but only that we prefer one logic to another.

Dr. George points out how the conception of pattern-making runs through human actions, in particular children's action, adult irrational actions, and games. Many actions appear to be attempts to complete incompletable patterns. He makes an important point of the comparative irrelevance of measurement to science: pattern-properties depend upon the idea of order rather than that of quantity.

One or two miscellaneous points of interest may be mentioned. Dr. George does not agree that great discoveries are the inevitable outcome of vast masses of data unearthed by a host of observers. The knowledge of these must pass through a single mind which can contribute a certain

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pattern before advance can be made. If discovery was an inevitable result of researches "we must suppose that a man like Faraday not only worked but also slept under the Tree of Knowledge, as he seemed to be always there when the fruits were falling" (266). There is an interesting discussion on the differences between an atom and a chair. It is pointed out that the General Theory of Relativity is not a theory of gravitation. It is a pattern and gravitation is another *pattern*. That is, gravitation is not a phenomenon to be explained but is a proposed explanation of certain phenomena. Certain physicists in the past attempted "to explain not the phenomena of gravitation but gravitation itself". There is a most interesting section dealing with the cost of and endowments for research at universities and other places; and a very clear tabulation of the characteristics of scientific theories and postulates.

Dr. George puts his case so very ably from his own special angle that criticism on points of detail is out of the question. One either accepts his view of Scientific Method or one does not. To criticise his view at all would amount to passing judgment on the general nature of induction. After reading this book one is left with a desire to know why the patterns that scientists weave are so successful; but this is a question that, as a scientist in action, Dr. George has no obligation to answer.

J. O. WISDOM.

L'Année Psychologique. Publiée par HENRI PIÉRON. Trente-quatrième Année (1933). Paris, Librairie Félix Alcan, 1934. Pp. xxvii + 1165. 120 fr.

As in recent years this number appears in two parts with continuous paging. In addition to the usual valuable abstracts of psychological literature, there are eight original papers. Under the title 'The Vibrotactile Senses' (*Les sens vibro-tactiles*) Robert H. Gault describes his well-known investigations into the possibility of making the deaf 'hear' speech through the palms of their hands or the tips of their fingers. He shows that by means of a specially devised 'receptor', an amplifier, and a 'vibrator', deaf subjects can be got to apprehend spoken words and phrases with notably greater efficiency than by lip-reading. In concluding the paper he suggests the possibility of a special 'vibration sense', a possibility which has also been suggested by Katz. An interesting paper on vowel sounds (*Les sons de voyelles*) by Kucharski might be regarded as more or less supplementary to that by Gault. In this paper Kucharski first discusses the various theories of vowel sounds, in particular those of Helmholtz, Hermann, and Paget. He then describes experiments of his own which go to support the theories of Paget. Briefly these experiments show that vowel sounds can be produced synthetically by combining two tones a definite interval apart, the interval varying with the vowel sounds.

Two papers are devoted to problems of learning. The first, by Foucault, on the influence on retention of the interval between repetitions (*Les intervalles entre les lectures et leur influence sur la fixation*) attempts to answer two questions: (1) What is the law in accordance with which retention value depends on the magnitude of the intervals between repetitions in learning? (2) What is the source of the retention value of intervals? The answers to these questions are neither definite nor very satisfactory. What Foucault actually finds is that up to an interval of 60 seconds between repetitions the learning efficiency increases, and thereafter

diminishes. Two laws, he says, will express this result, the first that the increase is in accordance with the ascending branch of a hyperbola, the second that the decrease is in accordance with the descending branch of a hyperbola. These laws do not seem to tell us very much, but the facts established are interesting. The second, by A. Zaganczyk, on 'The Effect of Deferred Reward on Learning' (*L'effet de la récompense différée sur l'apprentissage*) is in the main a criticism of Thorndike's 'Law of Effect'. The general conclusions are: (1) that the effect appears to be only slightly influenced by the duration of the delay, and (2) that when the delay involves a mental operation of some difficulty its influence is more marked. A conception of the 'Law of Effect' essentially psychological, rather than mechanical, is implied, the author thinks, by all experimental results.

A short paper by R. Dellaert on the intelligence of moral deviates (*L'intelligence des anormaux du caractère*) is of considerable interest, especially in view of the attention at present being devoted to delinquency. Tests were given to 53 inmates of the Institut Médico-Pédagogique Saint-Jozef Zwijnaarde-lez-Gaud. These were about fifty per cent moral defectives, and fifty per cent. unstable. Both verbal and performance tests were employed—the former being the Binet-Simon, and the latter the Pintner-Paterson, Healy Picture Completion II., and the Porteus Mazes. Generally speaking the subjects were found to show marked intellectual inferiority—27 of the 53 being mentally defective. A very interesting finding was that performance tests seem to have a higher diagnostic value in this field than the Binet tests.

The other three papers, *Le temps d'action des accroissements de brillance juste perceptibles* by Durup and Piéron, *Le Problème du mécanisme physiologique impliqué par l'échelon différentiel de sensation* by Piéron, and *La conduite psychologique devant l'effort mental imposé* by S. Korngold and A. Lévy, are also important, but rather narrow in their appeal and highly technical.

Two short notes are added, the first by Weinberg and Fishgold on the variation of the auditory threshold as a function of the initial intensity of the stimulus, and the second by Piéron on the perception of apparent movement.

J. D.

The Marks of Examiners, being a Summary of Investigations on the Comparison of Marks allotted to Examination Scripts by Independent Examiners and Boards of Examiners, together with a Section on a Viva Voce Examination. By Sir PHILIP HARTOG, K.B.E., C.I.E., and E. C. RHODES, D.Sc., with a Memorandum by Prof. CYRIL BURT, M.A., D.Sc. London: Macmillan & Co., 1936. Pp. xix + 344. 8s. 6d. net.

THIS product of the International Institute Examinations Enquiry contains the material already published in cheap pamphlet form as *An Examination of Examinations* in 1935, at 1s. net, and fully reviewed in MIND for July, 1936 (No. 179), which is now already in a fourth impression. This fact alone establishes that it has excited widespread and well-deserved interest, and it is quite probable that most of those interested will content themselves with the pamphlet form of these important researches. But the Carnegie International Institute which has sponsored them may have been right in thinking that to make a lasting impression on the academic mind it was necessary to pad and expand its results into this stately volume. So it has equipped them lavishly with tables and curves and

equations and all the paraphernalia of mathematical 'analysis,' somewhat after the fashion of the schoolboy who explained the co-existence of the three Creeds by declaring that the Apostles' Creed was found to be too simple to carry conviction, and even the Nicene Creed was not sufficiently impressive, "so they invented the Athanasian Creed, and no one could help believing that!"

The actual examinations investigated were (1) School Certificate History, Latin, French, Chemistry, and English; (2) the Special Place Examination, on which are awarded the Scholarships forming the educational ladder that leads from the primary to the secondary schools, as regards the marking of the arithmetic papers and the English Essay; (3) the College Entrance Scholarship English Essay; (4) University Honours examinations in Mathematics and in History; and lastly (5) A Viva Voce Examination of the sort demanded for entry in the Public Services, for which a £100 prize was offered and 16 students "with excellent University records" were selected out of 30 applicants. It can hardly be denied that these are all important examinations, and form a fair sample of the national examination system. Also it is made quite plain that the conditions of the test were far more conducive to good examining than those of the actual examinations. The pay was better, there was no need for hurry, and the examiners were all experienced men of established reputation, who were, moreover, supplied with very detailed instructions.

The aim of the examination throughout was to test the 'consistency' of the examiners rather than the 'validity' of the examination itself, i.e., its value for educational purposes; in view of the great variety of the examinations it would not have been at all surprising if the results had differed greatly in different subjects. But alas, as a matter of fact, the results showed that in no case could the examiners agree, and so far as the consistency of examiners is essential to the value of an examination all examinations must be condemned. Not that there were not considerable differences as between the different subjects: School Certificate Latin and French did more or less agree on easy questions of grammar, but even in the sciences agreement proved unattainable and human idiosyncrasy defeated the presumption of scientific 'exactness'. As the authors remark (p. 243) "the general idea that mathematics and science subjects can be marked with greater precision than humanistic subjects is apparently not founded on a sound basis". On the other hand they admit that "least precision is possible in Essay type examinations" (*ibid.*). The severest condemnation is, however, reserved for School Certificate History, where the irregularities are said to be "so large as entirely to discredit the value of the test" (p. 338). The philosophers must account themselves very fortunate that their examining was not inquired into, else they might have provoked even harsher comment.

Where, then, shall the ideal examination be found? Not in all probability in the examination systems of England or France or America. But research might profitably be bestowed upon the now lamentably defunct Civil Service examination of China. For it has an unquestioned and unequalled record. For over 3000 years, until it was suppressed in 1911, it selected the best brains in China and kept that great country civilised, orderly, and pacific. If the secret of its success proves impenetrable, we may have to wait until Dooms Day for a just assessment of our merits. But will even the Recording Angel resist all temptations of tampering with the marks in order to attain the smoothness of a perfect curve?

F. C. S. SCHILLER.

An Essay on Economy and Value ; Being an Enquiry into the Real Nature of Economy. By ALEC L. MACFIE. London: Macmillan & Co., 1936. Pp. xi, 152. 7s. 6d.

SUPPOSE you ask what it is to economize. You will then find yourself in a field pretty accurately denoted by Prof. Robbins's definition of economics, i.e., "Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses." You will also find, however (if the problem gets a grip of you) that there has to be an economizing of ends as well as of means, and that you are making the question a good deal simpler than Providence meant it to be if you assume that ends and means in a man's life can always be kept rigidly separate. Indeed it seems to me highly probable that you will reach a view very like Mr. Macfie's in its general outlines, viz., that three levels should be distinguished. "The first of these is positive economic science, the second is social economic science, now normative; and the third is a glorious, but neglected, hybrid, which really ought to be worked out by social philosophers, but which can perhaps best progress in actual circumstances through both economists and social theorists reaching out towards it and striking out fire by rubbing against each other" (p. 80).

Mr. Macfie develops his thesis in a style that is better than merely pleasant, and he has a gift for apposite illustration. I am inclined to think that he is over-hospitable to ethical theories, taking something from nearly all of them provided that they are axiological and not deontological. Such a household would be very difficult to live with. This defect, however (if I am right in believing that it exists) is of no great consequence in a general sketch that is meant to illustrate rather than to define the attitude of ethics to the problem. On the other hand I seem to discern a certain weakness in some of the arguments that he rightly takes to be central. It is a mere *non sequitur* to say (p. vi.) that "the view that economics is only positive leads straight to the vicious practical conclusion that our bitter social evils can be cured by mere economic rearrangements." Again I am sorry that he was moved to argue not only (correctly) that economizing is essential to any axiological pattern or system but also (dubiously) that economizing is an end in itself because "we obviously enjoy being efficient for its own sake" (p. 39). He himself remarks, in the very next sentence but one, that there is an efficiency of destruction. He might have added that this, even if wanton, is often very enjoyable.

I may, however, have exaggerated the importance of such arguments in Mr. Macfie's thesis; and in any case should like to express the opinion that moralists and social philosophers should make a point of reading this stimulating little essay.

JOHN LAIRD.

Essais. Par ÉMILE MEYERSON. Paris, Vrin, 1936. Pp. xvi, 273. Price 32 f.

BEFORE he died M. Meyerson expressed a desire that these essays should be published in a single volume. The three earliest, belonging to the period 1884-91, show him as a pure historian of science. There is one dated 1914, in which he uses his knowledge of the history of science in a

discussion of a historical generalisation of Weber. The remaining essays fall into two sets, three printed in 1923 following on the publication of *De L'Explication dans les Sciences*, and five published in 1934 following on *Du Cheminement de la Pensée*. In the essays of 1923, he was concerned to illustrate the general principles which he had expounded and defended in *Identité et Réalité* and in *De l'Explication*, and to show the fruitfulness of their application to special problems in philosophy and psychology; while in those of 1934 his object was to defend his views against various criticisms and misunderstandings, to set in the clearest possible light the method he followed in his investigations, and to justify this method as the only satisfactory one for the solution of his special problem.

The two central essays of the volume are *Philosophie de la nature et philosophie de l'intellect*, and *De l'analyse des produits de la pensée*, the former contributed to the *Revue de Métaphysique*, the latter to the *Revue Philosophique*. Taken together, they give a very full and clear account of Meyerson's general problem and of his method of dealing with it.

In the essay on *Les Mathématiques et le divers*, Meyerson defends his view of mathematics as depending on a diversity presented by the real, and as being, like any other type of thinking, an attempt to reduce this diversity to identity; and in *La notion de l'identique* he brings out the importance for his thesis of taking identity in the most rigorous sense as excluding all diversity. The completely rational is the completely identical; and the history of man's efforts to understand the real is the history of his attempts to refer the diversity characteristic of sensation to an objective source from which all diversity shall be excluded. Meyerson was aware of the paradoxical appearance which his thesis presented; for without diversity thinking would not be possible, while (on his thesis) with diversity thinking is not rational. But the wider and more thorough the survey he made of man's intellectual efforts, the more convinced he became that they could only be understood if regarded as strivings after the unattainable ideal of pure identity.

In the preface M. de Broglie pays a fitting tribute to Meyerson's personal qualities, and to his work.

L. J. R.

Das Verhältnis der Kategorienlehre zur formalen Logik. By OSKAR FECHNER.
Rostock: Carl Hinstorff, 1927. Pp. 175.

THIS book undoubtedly shows a great deal of ability, but the ability seems to me rather misapplied. The main part of the work is occupied by a list of categories running into dozens and a corresponding list of "*Kategorienformen*," i.e., *Kategorien ohne Gültigkeitsgrund*. In practice this amounts to a list of the various formal relations that can with sufficient ingenuity be derived from the notions of identity, diversity, ground, class. The categories have little connection with Kant. We must regard the subtitle "*Ein Versuch der Ueberwindung Immanuel Kants*" as misleading. About the value of the actual list given I find it very difficult to decide. I am well aware that very probably there are many points which I should appreciate better if I had more knowledge of relevant philosophical literature in the country where the book appeared, but I doubt whether most English readers would find much point or value in many of the repeated distinctions. In any case the work is often very lacking in clearness. Examples certainly ought to have been given with each category

and "Kategorienform," and when technical terms none of which have been adequately explained are piled on the top of each other as in "*ideal—ontologisch—intra—typisch—phaenomenhafter Bogen*" it becomes quite impossible to discern clearly what the author means. And the relevance he thinks his logic has to philosophical problems is not made clear. *E.g.*, he says that he has deduced¹ from his logical investigations a conception of organisms like that of Driesch, when in fact he has merely stated what he thinks to be the equivalent of the Drieschian view in his own formal language without giving any evidence for it that I can see.

His general account of logic at the beginning is clearer and has some merits, but it is very strange that, although he obviously has read books written by the more modern school of logicians, he yet without any argument in defence of himself explicitly assumes the view that all judgments should be regarded as being of the subject-predicate form² and explains existential judgments as asserting the relation between the subject and existence.³ To the modern "logistic" he objects that, since Logic studies concepts and concepts are thoughts by the subject, the objects of logic must not be put in the same class as those of mathematics, and that, therefore, modern logicians who treat relations and classes as the objects of logic confuse thoughts with the objects to which thoughts are directed. But, if this be a valid objection, I cannot see that his own account escapes it.

I have devoted most of my review to grumbling at the book, but this is partly because the author clearly had enough ability to make it so much better—*corruptio optimi pessima*—and I should still recommend anybody who is pursuing research in either formal logic or the theory of relations to consult it. For less specialised students I could not recommend the book, but then I do not suppose it was intended for this wider class. However, even for research students it is extremely regrettable that the author did not continue with the same clarity as he showed in the first thirty or forty pages. There ought to be an index.

A. C. EWING.

Les Hallucinations. By PIERRE QUERCY. Paris: Félix Alcan, 1936. Pp. 179. 10 Fr.

THE author, a physician at the asylum in Bordeaux, and a lecturer in the university of this same town, very naturally treats his subject from the medical point of view. He uses the term 'hallucination' in a wide sense, so as to cover 'illusion' and many features of normal perception, and is chiefly interested in the classification of the phenomena and illustrating them, for choice from his own observations. These are described and referred to with commendable conciseness; but the fact that no attempt is made to write them up, produces the impression of a certain dryness. In part this is no doubt due to the fact that Dr. Quercy has also treated his subject, more amply, in two volumes; in fact his present little volume may well be merely a convenient condensation of his larger work.

F. C. S. SCHILLER.

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¹ Pp. 165-166.

² P. 12.

³ P. 14.

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VIII.—PHILOSOPHICAL PERIODICALS.

JOURNAL OF PHILOSOPHY. xxxiii., 21. **F. J. E. Woodbridge.** 'The Problem of Consciousness Again.' [Suggests that "the debate about consciousness is a typical case of philosophical absentmindedness, like looking for one's spectacles to find them on one's nose. . . . To be free of the habit of turning perception—being conscious—into a problem of knowledge is to be wellnigh free of the problem of consciousness altogether."] **E. Vivas.** 'A Note on Value.' [Calling value any object of any interest, it is yet proper to distinguish between the *desired* or 'value in isolation' and the *desirable* or 'value in systems'. "Value is defined in terms of the rule which governs our system of interests," but "comparative judgments of value depend upon the existence of common interests in the values under comparison". If then "two men mutually reject the rule of each other's system, and if no common ground can be found on which a comparative judgment between the values at issue is possible, no rational discourse about value is possible."] **S. C. Pepper.** 'On the Cognitive Value of World Hypotheses.' [Replies to E. W. Hall in xxxiii., 9, that even though no metaphysics may be absolutely true yet some may be better than others.] **E. J. Nelson.** [Criticizes Hans Reichenbach's claim, in xxxiii., 6, to have solved the problem of induction, and concludes that "I am at a loss to see how Prof. Reichenbach has solved the problem of induction or how he has answered Hume."] xxxiii., 22. **M. ten Hoot.** 'Awareness and Inference: An Approach to Realism.' [Arguing that "perception, when not disturbed by philosophic doubt, has always functioned in an atmosphere of animal faith", and that there is not "any sound practical reason why this animal faith needs to be disturbed", the author infers that "no practical issues are involved in theory of knowledge". Thence he proceeds to advocate a 'realism' "which asserts that the existence of the external object can be 'inferred from' perceptual contents".] **L. P. Chalmers.** 'Plato's Objective Standard of Value.' [Starting from a mistranslation of a well-known passage in the *Republic* which transfers to the world of sensible existence what Plato asserted of the inter-relation of his Forms, the author interprets the universals as "creative processes" in a theory of 'evolution', and the Idea of the Good as a "nisus to unity." In this way he hopes to preserve the absoluteness of the values, truth, beauty, and goodness, and to save democracy.] **I. Edman.** 'Poetry and Truth in Plato.' [Discusses whether Plato is more truly taken as a metaphysician or as a poet, and concludes that he "is ultimately driven back to the poetry he had deserted".] xxxiii., 23. **W. Gruen.** 'Determinism, Fatalism and Historical Materialism.' [When a scientific 'law' is taken methodologically it has a reference to the future and is a guide to future action; and "universals are terms of discourse expressing ways of thinking about change". Necessity, on the other hand, is a principle of explanation for absolute fatalisms (Augustinian grace,

Calvinistic predestination, Mohammedan kismet) and absolute determinisms (absolute idealism, in short), for monisms. In pluralism, necessary and contingent beings can be distinguished. "The contingent is always a novelty, and conversely no novelty can be necessary." Hence "no monistic metaphysics can serve as an adequate foundation for a philosophy of history". Moreover, in the absence of methods for specific predictions fatalism as little leads to predictable knowledge as thorough indeterminism. However "the universe is not a single determinate system: it is simply the locus of all possible systems", and "the historical event is an abstraction from the concrete situation to which it refers". Thus "necessity is a category of logic, not of the natural or social sciences". In scientific use, moreover, it does not follow from *if A then B* that B must occur: it may fail to ensue from *if A and C*, and C may be a 'chance' event for any given system. It is not an uncaused event, but an 'accident'. Hence "in the physical world nothing is inevitable", and the predictions of economic determinism need not come true.] **C. H. Kaiser.** 'The Continuity of Change, I.' [Starts by criticising Whitehead's *Process and Reality* and protests against his 'atomic occasions', but prefers him to Einstein, and concludes that for the existence of a causal relation between events A and B (1) each must be "merely a phase in the active pattern of event C which extends over both", and (2) "the active pattern of the causal event must be reiterated in every event of the route connecting A and B."] xxxiii., 24. **C. H. Kaiser.** 'The Continuity of Change, II.' [Proceeds to analyse 'percipient events', and concludes that (1) "nature is a . . . tremendously diversified, uncompleted event which is not in time and space, since these, in their purity, are merely abstractions from the concrete world of process": (2) "Our world is essentially unfinished, essentially dynamic and yet essentially one": (3) The relation of extension and the notion of active pattern guarantee the continuity of change and becoming: (4) There is provision for almost infinite complexity.] **A. N. McLeod.** 'A Problem in Philosophy.' [Zeno again!] xxxiii., 25. **J. Dewey.** 'General Propositions, Kinds and Classes.' [Demands "discrimination in logical theory between existential objects and logical and mathematical objects", but accepts the 'conventions' that A and E propositions do not imply (or refer to or postulate) existence, whereas I and O do, as also the convention of formal logic that some terms cannot become ambiguous. Nor is any more radical remedy suggested than that a distinction should be drawn between a 'class' and a 'kind'.] Abstracts of Papers to be Read at the Thirty-sixth Annual Meeting of the Eastern Division of the American Philosophical Association, Harvard University, December 28-30th, 1936. xxxiii., 26. **D. C. Williams.** 'Tokens, Types, Words and Terms.' [A somewhat arid disquisition on the 'clarification, and analysis' which are said to be 'the alpha and omega of philosophy'. The author believes that "such feats are very slight and menial parts of philosophy", but feels that he too must qualify.] **N. P. Stalknecht.** 'Semblance and Substance in Aesthetics.' [Has the sub-title "The Place of Responsibility in Art and Morals", and raises the question "can the term 'beautiful' be employed in criticism of the arts, but also of moral actions and decisions?" But as the author does not appear to have realised that the Greek ethical terminology is throughout aesthetic in type, he does not get very far.]

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PROCEEDINGS OF THE ARISTOTELIAN SOCIETY. New Series. Vol. xxxvi. 1935-1936. **J. L. Stocks.** Presidential Address: 'The Need for a Social Philosophy'. ["If there is scepticism and disorder in the practical field"—i.e., the field of morality and social organisation; the writer seems to exaggerate the extent of the disorder—"the philosopher is totally unable to remedy it and reconstruct the shattered fabric of belief. . . . The very nature of the philosophic task precludes him from any such service". What he can do is to assist men of action by analysing for them, and expounding to them, the fundamental beliefs implied in their activities.] **H. C. Dowdall.** 'Corporate Personality psychologically regarded as a system of interests.' [The writer is keenly interested in the problem of corporate personality and proposes to inquire how far psychology can throw light upon it; and, as he has a high admiration for Ward's psychology, he begins by giving an outline of its main doctrines. The conclusion, however, which he carries over to illuminate his problem is of a rather general kind, viz., that a "society is a going concern, if each member is disposed, for any reason whatever, to perform the task allotted to him by the laws and orders of the society". That is to say, the life of a society consists in the working together of the dispositional interests of its members. With this view of the life of a society it can only be by a misunderstanding that the writer thinks himself compelled to reject the doctrine of a general will as held by Hegel and Bosanquet.] **J. I. McKie.** 'A Note upon Time and Cause.' [The course of this 'meditation' cannot conveniently be summarised and in fact is none too easy to follow. Perhaps it may suffice to quote the final result. "An actual Universe which contains change must be a developing continuous whole, composed of individuals distinct but not utterly discrete, and such a Universe cannot help but be rational, in the sense that the pattern of its development must exhibit a recognizably individual system. . . . Since it is part of the very notion of the general, that its particulars must give each its own twist to their manifestations of it, causal necessity can never eliminate from the course of history its unpredictable novelty. . . . Hence in this Universe, ruled as it is by Law, there is not merely room for Freedom. It is indispensable."] **J. O. Wisdom.** 'Towards the Psycho-centric Conception of Right.' ["All action," the writer holds, "has its most important source in the unconscious." Readers who are prepared to accept this assertion may perhaps be able to take the paper seriously, but it is unlikely that any others will.] **C. E. Joad, A. C. Ewing, A. M. MacIver.** Symposium: "Is there Mind-Body Interaction?" [Joad proposes to consider three solutions of the mind-body problem: "(1) that there are only bodies, (2) that there are only minds, (3) that there are minds and bodies and that they interact". One would have thought that (1) and (2) were excluded by the terms of the question in debate. As to (3) he accepts interaction as a fact, but can suggest no 'solution'. He argues, however, that causation is no less mysterious and yet is accepted as a fact. Ewing says there is no fundamental disagreement between himself and Joad, and, after commenting on the latter's paper, goes on to answer (not very convincingly) the arguments against interaction. MacIver startles the reader by proclaiming that every thinking thing is extended, but perhaps he intends merely to disclaim the notion of mind as an entity separate from its body. He goes on to argue *per contra* that extended things may behave in a teleological or purposive way, and finally suggests that thinking "is essentially the adjustment of a teleological agent to those of its circumstances which are relevant to the achievement

of its purposes". Thus it is not very clear what his real position is.] **A. H. Hannay.** 'Is the Imagination Creative?' [An examination of the meaning and function of 'imagination' in science, history, and the arts. Popular views on the subject are incidentally criticised.] **Bertrand Russell.** 'The Limits of Empiricism.' ["We all in fact are unshakably convinced that we know things which pure empiricism would deny that we can know." "Experience gives more information than pure empiricism supposes." This is shown by an examination of various cases, *e.g.*, it is argued that the doctrine of finitism in mathematics involves an exaggerated empiricism.] **L. J. Russell.** 'Ought implies Can.' [The writer is not content with the conception of freedom which says that a person could have done otherwise if he had wanted to do so. Accordingly he tries, by an analysis of what is implied in universal causation and of the conceivable alternatives, to arrive at a view that will make room for a more real kind of freedom. The conception which he reaches is that of an agent having some properties which are indeterminate within limits. He does not deal with the obvious difficulty of understanding how anything can *exist* in an indeterminate state.] **H. J. Paton.** 'Kant's Analysis of Experience.' [The paper is in the main expository and states in broad outline the argument of the first half of the KRV, but occasionally the agreements or difficulties of the writer with Kant's argument are mentioned. Thus he says that he can see "nothing impossible, or even improbable, in the theory that human sensibility and human thinking impose certain necessary characteristics on all objects as known to us". Some readers may have more difficulty in accepting the theory or perhaps even in finding it intelligible.] **Helen Knight.** 'The Use of "Good" in Aesthetic Judgments.' [The main contention of the paper appears to be that when the term 'good' is used in aesthetic judgments it expresses, not mere subjective liking, but an objective quality estimated in terms of specific criteria applicable in each art and in each type of artistic work within the art, although it may also be the case that goodness in one type counts for more than goodness in another.] **E. F. Carritt.** 'Hegel's Sittlichkeit.' [Hegel's conception of Sittlichkeit is made the object of an attack for which his own criticisms of Plato and Kant are taken as starting-points. It is argued that in the very respects in which he criticises them, his own conception is inferior. At the beginning of the paper the writer tells us that "in the only sense which it seems legitimate to give to Hegel's doctrine" he finds it "almost incredible that anyone should have entertained it". One can hardly but infer that it is quite incredible that the writer can have grasped the true meaning of the doctrine.] **Beatrice Edgell.** 'Conceptual Recognition.' [The paper is taken up almost wholly with a historical review of opinion from Locke to Spearman and other present-day psychologists. The writer's own view is not made clear except in so far as she seems to connect with approval Stout's psychological doctrine of relative suggestion with his view of universals. "The universal of conceptual recognition is distributive."] **D. R. Cousin.** 'Some Doubts about Knowledge.' [An examination of the view of knowledge taken by disciples of Cook Wilson. It is argued that they identify two quite distinct meanings of knowledge, *viz.*, (1) certainty—and this, the writer maintains, depends always on tautology; (2) direct presence to the mind—and in this meaning he maintains that it is the object of perception, rather than the sense-datum, that is directly apprehended.]

ARISTOTELIAN SOCIETY, SUPPLEMENTARY VOLUME, XV., 1936: WHAT CAN PHILOSOPHY DETERMINE? **G. F. Stout**. Inaugural Address: 'Universals again'. [In this address Stout returns once more to the defence of his doctrine of universals. At the end he says that the difference between Moore and himself "may at bottom be one of language". No doubt it is largely so, but the natural usage of language seems to be against Stout, for surely we may say that two objects are of the same colour, and we do not then mean (as Stout would have us say) that they are of the same *kind* of colour. He speaks himself of reds 'of exactly the same shade'. But the present paper rather suggests that considerations about identity play a greater part in the dispute than might appear at first sight.] **H. H. Price, J. Laird, J. N. Wright**. Symposium: 'Memory-Knowledge'. [Price argues at length that it is impossible to maintain that all memory-judgments are fallible, and then discusses the relation of the memory-image to the past event. Laird contends rightly that all that Price proves in the first part of his paper is that "wholesale distrust of memory is absurd"; he then goes on to question the usual assumption that the memory-object must be contemporaneous with the memory-act. Wright first argues that what we remember is not, strictly speaking, an event, but rather the fact that an event of such and such a kind occurred, and then he in his turn discusses the relation of the memory-image to the original object.] **G. A. Paul, H. M. Smith, A. R. M. Murray**. Symposium: 'Is there a Problem about Sense-Data?' [Paul shows very well how needless and how misleading the notion of sense-data and all the phraseology connected with it really are. The other two writers endeavour to defend the sense-datum but do not seem to appreciate sufficiently the argument that everything which this term is used to express can be expressed better without it. The following quotation from Murray's paper is enough in itself to show how the notion of sense-data may lead to a total misrepresentation of ordinary experience: "the fact surely is that when I sense a brown patch and am perceptually conscious of a penny . . . what I am judging is that the patch is a member of a 'family' of patches and other sense-data . . . in less technical language, . . . that in a different situation I *would* sense a brown patch of a different shape", etc. True, he says later that the plain man is not usually conscious that this is what he means, but the actual fact of course is that the plain man does not mean anything of the sort; the plain man's state of mind is much more simply and correctly expressed by saying that he sees a penny.] **C. D. Broad, A. J. D. Porteous, Reginald Jackson**. Symposium: 'Are there Synthetic *a priori* Truths?' [This symposium suggests the remark that, when there is any suspicion that the terms of the question proposed for discussion, or terms that are closely connected with them, may be ambiguous, the symposiasts should agree, if possible, among themselves beforehand as to the meaning to be given to the terms. Otherwise a great deal of time may be taken up with verbal discussions, and although these may not be wholly unconstructive, they are apt to be tedious and confusing, and they may seriously curtail the time available for the discussion of the real issues. Broad first says that *a priori* propositions must be either self-evident or demonstrable from self-evident premisses, and then points out that *a priori* propositions may be held to be not synthetical but analytical. The remainder of the paper is devoted to an inquiry into what can be meant by this doctrine as maintained by Ayer in a recent book. Porteous wants to use 'analytical' in the sense of necessitated by the definitions and postulates of

a system, and on the strength of this usage would partly defend Ayer but partly criticise him; finally he suggests that there are *a priori* propositions (e.g. causality) which are not analytical in the above sense. Jackson's paper is taken up for the most part with verbal discussions on points in the previous papers, possibly because he would have no hesitation in answering the question proposed in the affirmative. The symposium as a whole can hardly have fulfilled the expectations of those who proposed the question.] **W. Kneale, G. E. Moore.** Symposium: 'Is Existence a predicate?' [After a preliminary historical reference to Descartes and Kant, Kneale maintains that existence is not a predicate, and that the existential proposition must be dealt with on some such lines as Russell's; and then we get an exposition with the usual x 's and ϕ 's. In the next section some peculiar cases of existential propositions are dealt with. The last section (which is short) is intended to state a view about the nature of propositions as logical constructions. The last sentence of the paper invites quotation: "This is the explanation and justification of the doctrine that an existential proposition is an inexpressible adjunctive truth-function of the propositions of a range". The simple-minded reader may feel that, if this is what an existential proposition is, he understands it less than ever, and that, in fact, if he had to go by this account of the existential proposition, he would be hard put to it to know one when he saw it. Happily for the ordinary reader Moore's paper is less technical and more to the point. He shows by an appeal to the usage of words that there is clearly some difference between the use of 'exist' and the use of a word that expresses an attribute, e.g., we can say 'all tigers growl' but we do not say 'all tigers exist'. The second part of the paper is less clear, inasmuch as the usages of 'exist' with which he is there dealing are more dubious.] **J. L. Stocks, A. K. Stout, W. D. Lamont.** Symposium: 'Can Philosophy Determine what is Ethically or Socially Valuable?' [Stocks practically repeats the argument of the address printed in the *Proceedings*, and would confine the moral or social philosopher strictly to the task of the analysis and interpretation of practical beliefs and activities of whose rightness or wrongness it is not his business to judge. Stout, though largely in agreement, protests (with reason) against this rigid separation of interpretation from judgment about truth and error. Lamont agrees with Stout in holding that the philosopher is concerned with truth and not merely with interpretation, but disagrees with him as to the application of this view to the practical field, since ultimate valuations are not theoretical judgments but expressions of desire or preference. He has therefore to disagree with both Stocks and Stout in so far as they both go on the assumption that the task of philosophy in relation to theoretical beliefs and practical valuations must be the same.]

PHILOSOPHY OF SCIENCE. iii., 4 (October, 1936). **R. Carnap.** *Testability and Meaning.* [First part (51 pages) of a very interesting paper elaborating a modification of the positivist views associated with the Viennese circle. The important novelties in the earlier part fall into four groups. First, the *testing* of a sentence is distinguished from its *confirmation*: they correspond roughly to the procedure adopted in deciding whether a proposition is true and the result of the procedure, respectively. Thus "a sentence may be confirmable without being testable; e.g., if we know that our observation of such and such a course of events would confirm the sentence and such and such a different course would confirm its negation without knowing how to set up either this or that

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observation". Secondly, the principle of verification receives modification: "no complete verification is possible but only a process of gradually increasing confirmation," and in this respect "there is no fundamental difference between a universal sentence and a particular sentence with regard to verifiability, but only a difference in degree". Hence "confirmation instead of verification" is the test of meaning. Thirdly, a new solution of the problem of dealing with "disposition-concepts" such as visible, fragile, soluble, "which enunciate the disposition of a point or body for reacting in such and such a way to such and such conditions". Previous attempts to define a concept of this sort, say $Q_3(x)$, in terms of two known propositional functions, $Q_1(x, t)$ and $Q_2(x, t)$, t being the time variable, in the form:

$$Q_3(x) \equiv (t) [Q_1(x, t) \supset Q_2(x, t)]$$

are insufficient. For the definition of $Q_3(x)$ will be formally complied with if $(t) \sim Q_1(x, t)$, i.e., if the conditions in the protasis of the definiens are never satisfied. E.g., suppose c is a certain match which I burnt yesterday and $Q_3(x)$ means x is soluble. Let $Q_1(x, t)$ specify the conditions of a solubility test and $Q_2(x, t)$ the positive result. If c was never tested for solubility, we have $(t) \sim Q_1(c, t)$, thus $(t) [Q_1(c, t) \supset Q_2(c, t)]$ and hence, by definition, $Q_3(c)$. To avoid the paradoxical consequence that any object is soluble if no test for solubility is ever applied to it, Prof. Carnap now proposes that such terms, $Q_3(x)$, should be introduced by sentences of the form

$$(x) (t) [Q_1(x, t) \supset (Q_3(x) \equiv Q_2(x, t))],$$

or, in words, "if anything x is put into water at any time t , then if x is soluble in water, x dissolves at the time t , and if x is not soluble in water it does not". Sentences of this form (and slightly modified sentences in which the equivalence between Q_3 and Q_2 is replaced by implication) are called *reduction-sentences*. Fourthly, the notion of *reducibility of the confirmation* of a sentence S to that of a class C of sentences, is defined in such a way as not to exclude the case when S is not a consequence of any subclass of C but an infinite number of independent members of C are consequences of S . As usual in Carnap's papers, a careful distinction is made between logical and empirical questions. The early part of the paper under review consists of a careful elaboration of the above-mentioned definitions and their implications. In the remainder the logical analysis thus constructed is applied to the empirical analysis of confirmation and testing. Two undefined, non-logical, terms, "observable" and "realisable", are involved in the discussion. Of the first, the following rough explanation is given. "A predicate 'P' of a language L is called *observable* for an organism (e.g., a person) N , if, for suitable arguments, e.g., ' b ', N is able under suitable circumstances to come to a decision with the help of a few observations about a sentence ' $P(b)$ ', i.e., to a confirmation of either ' $P(b)$ ' or ' $\sim P(b)$ ' of such a high degree that he will either accept or reject ' $P(b)$ '." No sharp line is drawn between observable and non-observable sentences. The whole paper represents a very interesting transformation of positivist doctrine, in which hardly any thesis escapes revision or re-interpretation.] **A. F. Bentley.** *The Positive and the Logical*. [Vigorous and witty attack on the linguistic behaviour of logical positivists as exemplified in five contributions to *Philosophy of Science* by Carnap, Feigl and Blumberg. "We may ask whether men of their

manner of thought and action are on the way to enjoy successful and widening participation in the great human activities of knowing and interpreting the known." This paper's lively malice supplies the answer to the rhetorical question. When logical positivists have been shown to be waving magic wands, mis-using hyphens, spinning webs in a manner much nearer the mediæval than the modern, playing a logical keyboard in the pious wish that in time the right hand may come to know what the left hand is doing, inventing huge calculating machines and abusing a pure 'concept' which must come home in the end to roost on the wrist of its master, it is a surprise to learn, at the end of so comprehensive an indictment that with the "great objectives of the logical positivists" the author is "in fullest sympathy".] **F. B. Fitch.** *Physical Continuity*. ["What is the relation or relevance of mathematical continuity to physical continuity?" The latter is discussed somewhat obscurely, with special reference to Democritus. "Any object, indeed, is discontinuous in so far as it consists of sharply differentiated parts. The steel sphere itself, unless it is broken to pieces, does not consist of sharply differentiated parts; and this is all we mean in saying that it is continuous." The author then proceeds to identify continuity and homogeneity (individuality?). Irreducibility of the quantum of action is the ultimate basis of objective continuity of nature. "It is to be noted that this theory of continuity rejects the validity of the contradiction usually assumed to exist between atomism and continuity. On the contrary it maintains that continuity necessarily involves a kind of atomism. The atoms, however, are such that it is *meaningless* to divide or cut them, rather than impossible because of their solidity. This meaninglessness is expressed, for the quantum of action, by the indeterminacy principle." As for the connection between mathematics and physics, "the answer seems to be that all the laws of physics could be stated in terms of the rational numbers and without recourse to the infinitesimal calculus and the theory of limits, but the laws thus stated would be unnaturally complicated". And again "the relevance of mathematical continuity to physical continuity arises from the fact that physical continuity is ultimately a sort of indeterminacy and homogeneity introducing into our equations error factors that can be systematically minimised only by means of the method of limits of the theory of mathematical continuity."]

V. C. Aldrich. *Renegade Instances*. [While *a priori* propositions cannot be contradicted by negative instances, "according to current views, there are instances which, though they fall outside the range of significance of a proposition or system and hence cannot contradict it, do nevertheless tend to bring about a revision of it by a negative action of some sort. The purpose of this brief essay is to coin a convenient name for such instances and to describe certain properties of their peculiar negative action." To each *a priori* generalisation there corresponds an *a posteriori* generalisation, often expressed in the same words. When the latter is contradicted by a negative instance, the definitions, from which the 'necessity' of the former derives, may be changed. This is explained in detail with special reference to the example 'swans are birds'.] **J. Mayer.** *Pseudo-Scientific Economic Doctrine (contd.)* [Discussion of the suppositions underlying cost and utility theory. Classical economics "for the most part bears no resemblance at all to the facts of modern economic life" and "must be discarded in its entirety if a truly scientific approach to the problems of modern economic society is to be achieved". Extensive bibliographical references.] Reviews and Notes. Supplement:

H. B. Smith. *The Algebra of Propositions.* [Mathematical investigation of the problem of constructing all propositions in the algebra of logic "whose truth is independent of the form of the variables".]

REVUE DE MÉTAPHYSIQUE ET DE MORALE. xliii^e Année, No. 1. January, 1936. **B. Croce.** *La Poésie et la Littérature.* [The difference between poetry and literature is worked out in the context of a distinction between five modes of expression: viz., (1) passionate or sentimental; (2) poetical; (3) prosaic; (4) oratorical; (5) literary. Sentiment finds its natural expression in interjections, simple or elaborate. When intuition or imagination "transform" these into images and their corresponding words, we get poetical expression. When thought transforms these images into concepts and judgments, we have prosaic expression. And, lastly, the evocation of emotions for amusement's sake is the task of oratorical expression. These four types of expression are phases of a single spiritual activity which exhibits a circular movement, in that sentiment, passing into poetry and thence into thought and action, becomes sentiment again on a higher plane, so to speak, thereby providing fresh material for poetry, thought and action. Literary expression is not a distinct phase of this circular activity of Spirit, but belongs to all the phases mentioned, being, "like courtesy and savoir-faire", one of the forms of "politeness and culture", the function of which is to establish an "equilibrium" between the poetical and non-poetical modes of expression. There are four domains of literature, for sentiment, oratory, amusement, and instruction. The article concludes with a discussion of "art for art's sake", as a manifestation of the love of poetic expression for itself; of "pure poetry", which, so far as it does not fall under one of the above-mentioned four modes of expression, Croce classes with "magic and thaumaturgy"; and of poetry, non-poetry, and anti-poetry.] **G. Teissier.** *La Description mathématique des Faits biologiques.* [Illustrates the various problems that arise in the application of statistical methods to biological phenomena, and makes the important point that the choice of method must always be guided by purely biological considerations, if the results are not to be seriously misleading: biometry is a branch of biology, not of applied mathematics. It is tempting to assume that, for every value of the independent variable, the values of the dependent variable will show a normal, or near-normal, distribution; and that the formula thus obtained will hold at once for the whole group examined and for each individual within the group. But neither of these two assumptions may accord with the facts. *E.g.*, if we examine the correlation between age and body-weight in the growth of individuals of a given species, we may, with very large numbers, obtain an approximately smooth curve, suggesting that body-weight increases at a uniform rate with age. Study of each individual separately, on the other hand, shows that there are spurts in the increase of body-weight at certain critical times in the individual's growth, and a formula which disguises this important biological fact, is inadequate. Thus, more complicated formulæ and curves are required, if justice is to be done to characteristic features of the facts. Sometimes, again, several methods appear equally applicable. In that case, the decision on which is the "best", cannot depend on purely formal considerations, like simplicity, or fewness of parameters, but must be guided by the biologist's judgment of the biological importance of the phenomena to be represented. Of special interest are correlations which reveal "biological invariants", like the formula which expresses the correlation between the reaction of a muscle

or nerve and the intensity and duration of an electrical current which stimulates it.] Variétés.—**J. Gherea.** *Le Problème de la Connaissance et les Durées.* [Every object, as existing in time, has "duration", or, rather, is a duration. The universe, thus, consists of durations, conscious and unconscious. Behind the Self (*moi*), as distinct from the not-self, lies a conscious duration, the stream of consciousness, which is, initially, "impersonal". Each of us starts as such an impersonal conscious duration. The problem is, How is the judgment reached that there exist "other" durations in the world, "outside of", i.e., existentially distinct from, the duration which then comes to know itself as "I"? This judgment must be reached on the basis of experiences which form part of the duration to which the judgment also belongs. There is involved, therefore, a problem of "transcendence". The author offers no solution of the problem in this article, but refers to a bigger work in preparation. Instead, the article criticises the attempts to account for this transcendence which have been made by the "intuitionists", like Lossky, and other "realists" who hold that perception itself directly reveals the existence and nature of objects other than itself.] Variétés.—**A. Lautman.** *Le Congrès international de Philosophie des Sciences.* [Reports on the problems discussed, and the trend of the debates, at the Congress held at Paris in September, 1935. The central issue was between the nominalism upheld by the Vienna School, with its doctrine that all logical propositions are tautological, and the advocates of the thesis that systems of signs and the formal rules of their manipulation must remain in the service of the effort, scientific or philosophical, to grasp and express the truth concerning the real world.] Études Critiques.—**R. Marjolin.** *Liberté et Organisation, par Bertrand Russell.* [Reviews Russell's "Freedom and Organisation, 1814 to 1914" under three heads, viz., his account of the history of these hundred years; his philosophy of history; and his estimate of present tendencies in Europe.] Questions Pratiques.—**B. Mirkine-Guetzevitch.** *Corporatisme et Démocratie (suite et fin).* [Completes the defence of democracy (parliamentary government, majority rule) against the Corporative State. Democracy rests on the principles of the French Revolution and on the maintenance of National Sovereignty, both together yielding the ideal of a "social individualism". A number of contemporary attacks on democracy are passed in review and criticised. Believers in democracy must *move forward* in their efforts to apply democratic principles to the circumstances of present-day Europe. They must not accept the verdict that democracy is played out.] Supplement. Obituary: Maximilien Winter (a regular member of the editorial staff of the *Revue*). New Books, French and Foreign. Periodicals. Communication: Report on the Twelfth International Congress of Sociology. xliii^e Année, No. 2. April, 1936. **L. de Broglie.** *Réflexions sur les deux Sortes d'Électricité.* [A clear account of the development of our knowledge of electricity, and of the problems suggested by the present state of that knowledge. At first, positive and negative electricity were conceived as opposite, but symmetrical (like the left and right hands). Then the discovery of the electron and the proton with their very different properties suggested a fundamental dissymmetry. Lastly, the neutron and the positron were found, thus providing, apparently, four fundamental constituents of "matter". Problem: Can these four be reduced to three, and if so, how? Two possible hypotheses: Either the neutron might be formed by the union of a proton and an electron; or the proton might be formed by the union of a neutron and a positron. The consequences of each of these two

hypotheses are briefly developed.] **L. Lavelle**, *Être et Acte*. [This paper, it is interesting to note, was read both to the French Society of Philosophical Studies and to the German Society of Philosophy in 1935. It proclaims the "renaissance of metaphysical thought" in opposition to Positivism and Epistemology. The task of Philosophy, as Metaphysics, is to make us conscious, on the plane of reflection, of the Absolute which is always present in us and of which we are parts. Philosophy has to show us how every experience which we have is an experience of the Absolute; how every act which we perform is a participation in the ceaseless self-creation of the Absolute. The author replies vigorously to the various criticisms to which the doctrine of the Absolute is commonly subjected, and he claims that the familiar antithesis of subject and object, self and not-self, time and eternity, etc., can be solved only by understanding how these contrasted terms and the relations between them have their place in the unity of the Absolute. He rejects a pantheist version of the Absolute in favour of a monadistic one. The activity of each of us is a free participation in the self-creation of the Absolute, which, free itself, has created us within itself as free creators. The conscious realisation of this fact by philosophical reflection is itself the completest participation in the Absolute, as at once "Act" and "Being". A return to a Philosophy of the Absolute is the condition at once of seriousness of thought and depth of life.] **R. Berthelot**, *L'Astrobiologie et la Pensée de l'Asie: Essai sur les origines des Sciences et des Théories morales (suite)*. [Eighth instalment of this series of articles. Mainly concerned with the influences which have gone to the making of Pauline Christianity. Combats the current interpretation of St. Paul's doctrine as if it were an anticipation of Augustinian theology. Distinguishes what St. Paul retained of the teaching of Jesus and of the tradition among his disciples concerning the death of Jesus, and what he owes to the mystery cults of his time, and especially to Mithraism. "The mystery cults, in which the Pauline Christianity has sought inspiration and of which it is itself an example, are all derived from rites of agricultural magic; but these rites have become transformed in these cults into symbols in proportion as the growth of the plant from the seed is viewed as but a prelude and analogue of the resurrection and immortality of the human soul."] **J. Picard**, *Syllogisme catégorique et Syllogisme hypothétique (premier article)*. [First instalment of an article defending the categorical syllogism against the criticisms of logicians, like Goblott, who propose to substitute for it the hypothetical syllogism as better representing the actual movement of thought in general, and of scientific thought in particular as concerned with the connection of phenomena according to law. Traces the debate about the relative merits of the two types of syllogisms back to the arguments between the Stoics and the Aristotelians, especially concerning such questions as whether the hypothetical syllogism, lacking a middle term, is really a "syllogism" at all; whether, if it is, we can distinguish in it figures and moods corresponding to those of the categorical syllogism, etc. The author argues that, even when two propositions, like "All S is P" and "If S, then P", can for certain purposes be treated as *equivalent*, it does not follow that they are, therefore, *tautological*, or merely two different ways of expressing verbally the *same* thought. The categorical proposition and the hypothetical have different logical functions. The former is concerned with individual beings, with essences, with species and classes; the latter is concerned with laws and connections between diverse phenomena.] *Études Critiques*.—**E. Souriau**, *Sur les Moyens et la Portée d'une Esthétique*

de la Grâce : Notes méthodologiques à propos d'un livre récent. [Critical appreciation of Raymond Bayer's two volumes, *L'Esthétique de la Grâce.*] **M. Gueroult**, *Vers une Renaissance de l'Idéalisme allemand : la Philosophie d'Arnold Gehlen.* [An account of the neo-Fichtianism of Gehlen, with special reference to his book on the "Freedom of the Will".] Supplement. New Books, French and Foreign. Periodicals. Communication: Statement of the Organising Committee of the Ninth International Congress of Philosophy.

REVUE NÉO-SCOLASTIQUE DE PHILOSOPHIE. Tome 39. (Deuxième série, No. 52.) Nov. 1936. **A. van Leeuwen**. *L'analogie de l'être : Précisions sur la nature de cette analogie.* [The 'Analogy of being' in St. Thomas is strictly 'analogy of proportionality' (not the so-called 'analogy of attribution'). As such it has always a principal 'analogum', which is God: the various creatures are analogous with one another because each of them exhibits, at an infinite distance, 'analogy of proportionality' with some of the divine perfections. It is argued, with a conspectus of relevant passages, that rival views, which introduce a further 'analogy of proportion' into the relations between the creatures, misrepresent the mind of St. Thomas.] **A. D. Waelhens**. *Phénoménologie et réalisme.* [It is argued that the phenomenism of Husserl leads quite naturally, as can be seen in his latest works, to an 'idealism' of the type of Fichte or Hegel. This is a consequence of the "bracketing of existence", since a philosophical 'description' inevitably aims at getting behind its datum, and linking it up with an adequate cause; the 'putting of existence in brackets' has forbidden us to look for this cause in the object, and so throws one back on the 'transcendental subject'. Heidegger's so-called existential analysis, again, leaves the human subject the 'measure of all things', and thus proves to be nothing but an 'activist idealism'. Hartmann remains true to realism, but his reduction of phenomenism to the status of a propædæutic collection of *ἀπορία* amounts to a complete surrender of its claims.]

Critical Studies. **A. Marc**. *L'existence humaine et la raison.* [Deals with the (unfinished) *Sein und Zeit* of K. Jaspers. Jaspers escapes the complete 'immanentism' characteristic of Heidegger by more carefully distinguishing *Dasein*, the given datum, and *Existenz*, "that which always has still to be completed, the man as the individual and the man as the person". In many ways his views show a marked affinity with those of M. Blondel.] **L. de Raemaeker**, *Psychologie*; **A. Grégoire** and **G. Delannoye**, *Philosophie de la nature et des sciences*; **W. Goossens**, *Philosophie de la religion.* [Notices of recent works in these fields.] *Ouvrages Divers.* [Other brief critical notices of recent books.] *Chroniques.* [Les travaux de la Société philosophique de Louvain. Chronique de l'Institut supérieur de Philosophie. Chronique générale.] Répertoire Bibliographique.

KANT-STUDIEN. Band xl, Heft 4, 1935. Obit. notice, with portrait, of Elisabeth Förster-Nietzsche (d. Nov. 8), sister of the philosopher. **H. Heyse**. *Idee und Existenz in Kants Ethiko-Theologie.* [In its deepest meaning Kant's philosophy, through its emphasis on the primacy of the practical reason, is a secular metaphysic of experience, resting on life as *erlebt* in its totality. So understood, it is a critical philosophy in the sense of a philosophy of "crisis".] **J. Schwarz**. *Die Lehre von den Potenzen in Schellings Altersphilosophie.* [The central place it gives to will, its

interest in the unconscious roots of the conscious, its opposition to abstract rationalism, and its demand that philosophy should be devised for life, make the last stage of Schelling's thought congenial to present needs. The article considers his doctrine of potencies both in its origin and in its final form.] **P. E. Liljeqvist.** *Prinzipielles über Individuum und Gemeinschaft nach dem schwedischen Persönlichkeitsidealismus.* [On the ideas of Boström. Completes article in last number.] **W. Mönch.** *Marsiglio Ficino und die Nachwirkung Platons in der französischen Literatur und Geistesgeschichte.* [Traces the influence of Platonism in the romantic phases of French literature and finds in these a point of contact between the Platonic mind of the Germans and the predominantly Aristotelian mind of the French.] **Th. Peters.** *Euklid Elemente Buch X.* [Translation, with notes.] **W. Krampf.** *Die neueren naturwissenschaftlichen Entwicklungen in aktivistischer Wissenschaftsauffassung.* [A critical notice of the German trans. (1932) of P. W. Bridgman's "Logic of Modern Physics".] Notes—(a) on a signed entry in Descartes' handwriting (reproduced) in an autograph book in the possession of the City Library of Königsberg, an entry (it is here argued) probably made at Leyden in 1637; (b) on the reception of Kant's philosophy in the Roman Catholic circles of his day in Germany. Reviews.

Band xli, Heft 1/2, 1936. **F. Zadow.** *Kulturbewusstsein und nationale Wirklichkeit.* [Able comparative historical study of the specific genius of the French and German nations. Finds the difference chiefly in the persistence in the latter of its *Urerlebnis*, which seeks indeed Forms but modifies and breaks them when it finds them inadequate.] **K. Beurlen.** *Der Zeitbegriff in der modernen Naturwissenschaft und das Kausalitätsprinzip.* [Shows how the two features of the newer physics, the attachment of time to space and the denial of causality, are consequences of the reduction of time to an order of units of measurement. Since physical time abstracts from both irreversibility and qualitative filling, physical laws hold good only of an abstract world. It is not surprising, then, that when applied to actual events they involve themselves in remarkable mathematical complexities and even then make the events phenomena of chance.] **W. Krampf.** *Studien zur Philosophie und Methodologie des Kausalprinzips.* [Like the preceding makes even empirical science rest on *a priori* principles and includes causality among these. Physics is not competent to deal with the *quid juris?* of causality. A long and thorough article, directed chiefly against the positivism of the so-called Wiener Kreis.] **H. Pichler.** *Besinnung über Glück und Unglück.* **Th. Peters.** *Euklid Elemente Buch X.* [Continuation of translation.] Facsimiles of two letters of Kant's to Freiherr von Schroetter, Nov. and Dec., 1802, recently discovered in the Prussian Secret Archives at Berlin (Dahlem). Reviews.

IX.—NOTES.

À MONSIEUR LE DIRECTEUR DE LA REVUE "MIND".

24 Février, 1937.

MONSIEUR LE DIRECTEUR,

Les directeurs et rédacteurs des Revues philosophiques françaises dont les noms suivent sont extrêmement désireux de renseigner leurs lecteurs d'une façon aussi exacte et complète que possible au sujet du mouvement philosophique dans le Royaume-Uni. Or, leur tâche est rendue de plus en plus difficile par le fait que beaucoup de maisons anglaises d'édition n'envoient pas leurs livres pour comptes-rendus et même ne donnent pas toujours des réponses favorables aux demandes qui leur sont adressées par les Revues. Ils ont cherché de quelle façon faire entendre leur voix. Ils ont pensé qu'ils ne pouvaient mieux faire que de prier MIND de bien vouloir être l'écho de leur amicale plainte et de bien vouloir insérer cette lettre. Ils ne pouvaient trouver d'interprète plus autorisé que lui.

Grâce à votre intermédiaire, Monsieur le Directeur, ils espèrent pouvoir accomplir la tâche qu'ils s'assignent et donner à la philosophie anglaise, dans le tableau de la pensée, toute la place qui légitimement lui revient.

Ils vous prient, Monsieur le Directeur, d'agréer tous leurs remerciements et l'expression de leur vive reconnaissance.

Les Dir. des *Recherches Philosophiques*, H. Ch. Puech, A. Koyré, J. Wahl.

Le Dir. de la *Revue Des Sc. Phil. & Théologiques*, M. J. Congar.

Le Dir. de la *Revue Philosophique*, L. Lévy-Bruhl, de l'Institut.

Le Dir. de la *Revue de Philosophie*, Ch. Eyselé.

Le Dir. de la *Revue de Métaphysique et de Morale*, Élie Halévy.

Le Dir. des *Archives de Philosophie*, J. Souilhé.

Les Dir. de la *Revue de Synthèse*, H. Berr, L. Febvre, P. Langevin, A. Rey.

RÉPONSE À MM. GRELLING ET BETH.¹

Dans le numéro de MIND d'octobre, 1936, MM. Grelling et Beth se sont attaqués à mon article sur les paradoxes de la logique paru ici même, dans la livraison d'avril 1936.

Ils font, tous les deux, la distinction entre les paradoxes qui ne nous obligent à aucune modification des règles de la logique (et dont la solution

¹ V. MIND, vol. xlv., no. 180, pp. 481-488.

constituerait, d'après M. Grelling, tout au plus un bon exercice pour de très jeunes étudiants) et les "véritables" paradoxes. A ce propos, je dois signaler que M. Grelling range à tort dans cette dernière catégorie le paradoxe du menteur ; l'analyse de cette antinomie devrait être confiée plutôt à des novices en logique. Car, si je ne me trompe fort, M. Grelling s'abuse en croyant que la contradiction y résulte de la définition de la fausseté c-à-d. de la proposition "*la proposition p est fausse* équivaut à non-*p*" ; en fait, elle découle de la définition de "*q*", ce signe étant considéré comme le nom de l'expression "*la proposition p est fausse*". En effet, la définition de "*q*", tout comme celle du barbier, et exactement comme celle de la notion "heterologisch", sont des définitions inadmissibles ; constituant de fausses équivalences, ces définitions nous entraînent dans des contradictions.

Dans le travail annoncé dans l'article incriminé—et qui paraîtra incessamment sous le titre "L'équivalence, la définition, et la solution du paradoxe de Russell"—j'expose dans quelles conditions formelles une définition s'avère contradictoire ; c'est le cas, notamment, quand elle pose l'équivalence d'une fonction et de sa négation ou quand, formellement, il est possible de déduire d'elle une telle équivalence. Cet exposé constitue, me semble-t-il, une réponse suffisamment détaillée à la question de M. Grelling : "How can a definition be seen to lead to a contradiction by certain substitutions ? (p. 484)".

Quant au paradoxe de Russell, qui découle, lui également, d'une définition fausse, mais généralement admise dans la logique classique,—paradoxe considéré, pour cette raison, comme ébranlant les bases mêmes de la logique—je l'analyse dans le même article. J'y montre comment on peut l'éviter sans modifier les règles *fondamentales* de la logique, en se contentant d'amender la définition de "*ε*", pour qu'elle ne s'oppose pas au principe de contradiction. C'est cet amendement—qui constitue sans doute une modification, mais combien légère, de la logique classique—qui me permet de résoudre le paradoxe de Russell, et de répondre, en même temps, à l'objection de M. Beth.

Celui-ci affirme, en effet, que l'on ne peut pas résoudre les paradoxes *logiques* sans modifier les règles de la logique classique. J'avais déjà prévu cette objection dans mon article en écrivant (p. 207) que si l'on pouvait résoudre ces paradoxes sans modifier les règles fondamentales de la logique, une légère modification de la notion de classe s'aurait pourtant nécessaire. C'est par là, je l'avoue, que je m'écarte de la logique classique, mais c'est pour mieux observer le principe de contradiction.

CH. PERELMAN.

INTERNATIONAL CONGRESS OF PHILOSOPHY AT PARIS.

The President of the British Institute of Philosophy would be much obliged if members of the Mind Association who intend to be present at the Paris Congress, 1st to 6th August, this year, would communicate their intention to S. E. Hooper, British Institute of Philosophy, University Hall, Gordon Square, W.C.1.

MIND ASSOCIATION: ANNUAL MEETING AND JOINT
SESSION WITH THE ARISTOTELIAN SOCIETY.

THE ANNUAL MEETING of the Mind Association will be held this year at Clifton Hill House, Bristol, on Friday, 9th July, at 5 p.m.

It will be followed by a JOINT SESSION WITH THE ARISTOTELIAN SOCIETY, for which the following arrangements have been made:—

FRIDAY, 9TH JULY.

At 8 p.m. Chairman: Prof. L. S. Stebbing.

Address by Prof. G. C. Field: "On the Teaching of Philosophy."

SATURDAY, 10TH JULY.

At 10 a.m. Chairman: Prof. L. J. Russell. "Induction and Hypothesis." Miss Margaret Macdonald, Mr. G. Ryle, Mr. I. Berlin.

At 8 p.m. Chairman: Prof. G. C. Field. "Is there an Absolute Good?" Prof. W. G. de Burgh, Prof. C. A. Campbell, Prof. J. Laird.

SUNDAY, 11TH JULY.

At 10 a.m. Chairman: Prof. G. E. Moore. "Does Philosophy analyse Common Sense?" Mr. A. E. Duncan Jones, Mr. A. J. Ayer, Prof. L. S. Stebbing.

At 8 p.m. Chairman: Prof. H. H. Price. Address by Prof. C. D. Broad: "The Philosophical Implications of Foreknowledge."

Accommodation will be provided in Clifton Hill House.

The *inclusive charge* for board and lodging from Friday afternoon till Monday morning will be 33s. For *part-time accommodation* the charges will be: Lunch, 2s.; Tea, 1s.; Dinner, 3s.

There will be a charge of 10s. as a Registration Fee for Membership of the Joint Session. The papers will be published by the Aristotelian Society as a Supplementary Volume, which will be sent free of charge to all who have paid the Registration Fee. It is hoped that it will be ready in time to be distributed before the opening of the Joint Session.

In order to facilitate the making of arrangements, it is requested that applications for membership and accommodation should be made as early as possible. Payment of the Registration Fee and of the charge for accommodation should accompany applications, which should be made to: The Bursar, The University, Bristol.

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